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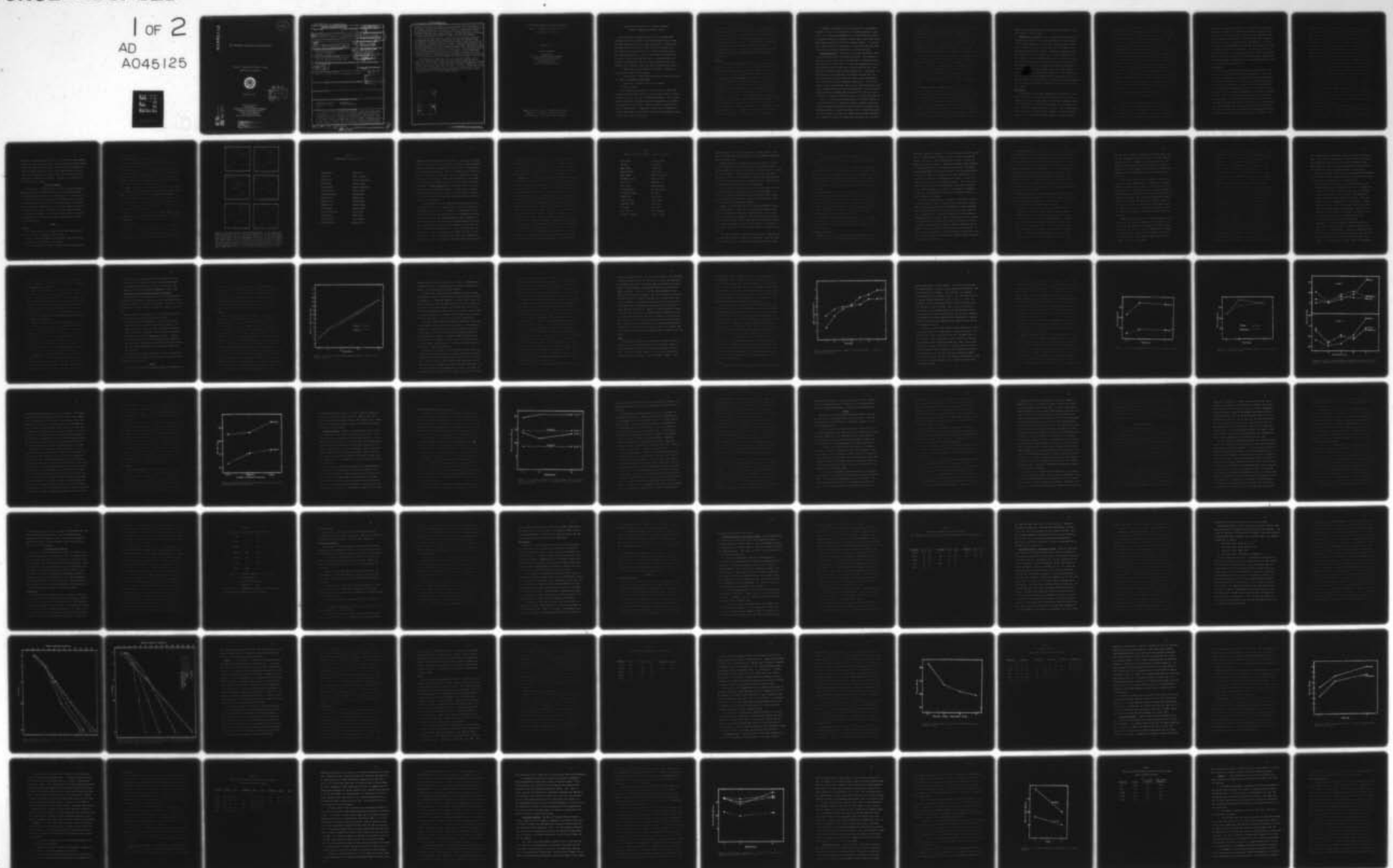

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THE SIMULTANEOUS ACQUISITION OF MULTIPLE MEMORIES

Benton J. Underwood and Robert A. Malmi

Northwestern University

September, 1977

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The basic paradigm required the subjects to learn four verbal tasks simultaneously. Each task was given a separate retention test (recall, recognition, frequency judgments, serial ordering). The materials to be learned were presented on a series of slides at a 20-second rate. Each slide carried stimuli from more than one of the tasks. The purpose of the research was to determine. (1) if the effects of certain intratask independent variables were the same as found when single tasks were learned; (2) the incidental learning which occurred when instructions specified the

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learning of less than the number of tasks presented; (3) the stability of the memories formed as determined by retention tests over a few minutes; (4) if differential encoding of the tasks occurred as a consequence of having different retention tests. The number of tasks specified to be learned differed, with four tasks always presented. In other conditions, the number of tasks presented varied as well as number specified for learning.

The results showed that the effect of certain intratask independent variables was much the same as found when the tasks are learned singly. For example, the spacing effect occurred persistently under a wide variety of conditions, including incidental learning. There was no measurable forgetting over several minutes for any of the tasks despite the fact that the retention intervals were filled with other memory tests. Incidental learning occurred, but it was not great in an absolute sense. Evidence for differential encoding appeared but it was not a consequence of the simultaneous learning of tasks because the same evidence appeared when the tasks were learned separately. As might have been anticipated, the rate of acquisition of any one task decreased as the number of other tasks being learned increased.

Manipulations of study time led to some puzzling findings. When a single task was presented, recall increased directly from 5 seconds of study time to 20 seconds of study time, but neither recognition nor frequency judgments was increased by study time beyond 5 seconds. Nevertheless, performance for the latter two tasks increased over trials. Furthermore, despite the fact that the performances did not increase as study time increased beyond 5 seconds, performance was better than when both tasks were learned simultaneously with a 20-second exposure period.

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The Simultaneous Acquisition of Multiple Memories

Benton J. Underwood and Robert A. Malmi

The research to be reported here was tied to an experimental paradigm that differs somewhat from paradigms commonly used to study the functional properties of the human memory system. Therefore, the procedures defining the central condition will be described at once. The subjects were told that they were to imagine they were taking a ride through an urban area. In such a ride (they were further told), it would be expected that many different events would be perceived, but that four particular classes of stimuli (events) were to be exhibited in the experiment. These were then described to the subjects:

1. Company names, all of which were fictitious, consisting of two words; for example, Victor Carpets.

2. Traffic signs, or signs calling attention to points of interest; for example, EXIT RAMP and CIVIC PLAZA.

3. State names as might be seen on license plates.

4. Names of streets.

These stimuli were presented on a series of slides. Each slide contained items from three or four of the four classes, and it is this fact which has led us to speak of simultaneous learning. In the central condition the subject was told exactly how learning was to be measured at the completion of the "ride." These measurements consisted of the recall of company names, recognition of traffic signs, estimating the frequency with which state names had occurred, and designating the order in which street names had been seen.

As will become more evident when details of the studies are presented, the procedures produced a difficult set of tasks for the subject. The perceptual-memory system was flooded, and if the subject attempted to encode the stimuli in a manner believed appropriate for the tests to be given for each class of stimuli, the confusion could be compounded. In a typical experiment, the subject is presented a single task, such as a paired-associate task. In the present procedure, the subject was presented four different tasks under instructions to learn them all simultaneously.

Purposes

The initial studies were undertaken as a result of a curiosity about certain possible outcomes. As we continued to manipulate certain factors, other questions and issues became of interest. In the beginning, however, there were four matters which engaged our attention.

Generality of memory phenomena. Most phenomena of memory as derived from laboratory studies might be described as aseptic phenomena. We normally choose (and properly so) to study them in as pure a form as possible, and this usually means making the situation as simple as possible. We try to remove all of the chaff we can; we try to reduce variability from all sources. In the present research, we have asked about the integrity of some of these phenomena when the subject's memory is being flooded with stimuli, many of which are not involved at all with the phenomena of interest. For example, all company names occurred twice. Sometimes the two occurrences were on the same slide, sometimes on different slides. Will a spacing effect occur in the recall?

It should be noted that we are not dealing with a confounding of variables. That is, the manipulation of the spacing variable is quite clean. It is simply being manipulated in a context where the subject is involved in acquiring information (traffic signs, state names, street names) that has nothing to do with the spacing variable. If a spacing effect emerges under such circumstances, it would seem that this phenomena could be described as being robust, or of high integrity.

Incidental learning. Our memories must include an enormous amount of information that has been acquired incidentally. This statement is not easy to document, but several different ways of viewing the matter would attest to its truth value. It seems quite unlikely that the primary content of memory consists only of events which have been accompanied by a personal command, "I must remember that." Furthermore, the long history of experimental work on incidental learning shows that incidental memory for certain tasks may, under certain circumstances, be as firm as will memories established intentionally. It is not our intent to review this work here; rather, we are interested at this point only in asserting that it does not seem unreasonable to believe that our memories would be of markedly different character if they were constituted only of information established under clear intentional conditions.

Still, even accepting the pervasiveness of incidental learning, it is not without interest to ask about situations in which some minimal amount of information processing occurs but in which no residue of this information remains in memory. Any radical view of a short-term memory as a mere holding system might also include the notion that information can be processed to some level without any consequence for a so-called long-term memory. In days not so long past, it was common to spend an afternoon in front of a calculator analyzing data. Colleagues of the senior author have pointed out that memory for numbers inserted in the keyboard, and perhaps written as well, was nil, and yet certainly the numbers were processed. Such a conclusion might be drawn from other situations. For example, in driving alertly in traffic, we are continually monitoring events occurring in front of us and behind us, and yet our memory for particular events is thought to be at best, poor.

In the present research, several conditions of incidental learning were included. For example, the subjects may have been instructed that they would be asked to recall only the company names so that their major task was to select out and learn those names. However, they were also tested for the other events depicted on the slides. It is presumed that in selecting the company names, perceptual processing (at some level) of the other classes of stimuli was necessary. Was there any information concerning the events in these other classes available in memory?

Stability of memories. How stable are the memories established under the overloaded conditions? Will we be able to detect a short-term component of memory that is extremely labile? The test conditions were so arranged that retention curves could be plotted over several minutes, and these intervals were filled with memory tests on other classes of material. Therefore, rapid forgetting might be expected if a short-term memory was heavily engaged, and also if output interference was involved. Furthermore, because all tests of incidental learning were given after the tests on material learned intentionally were given, whatever was learned incidentally would be said by convention to have been a part of long-term memory.

Independence of processes. The final matter of initial interest concerned the correlations among the performance scores for the tests of intentional learning for the four tasks. Assume a serious subject who, as instructed, tried to learn as much as he could about each set of materials within the time allowed. Substantial correlations among the performance scores could argue for a general learning factor or for a single process or mechanism underlying the learning of all tasks. If, at the other extreme, the correlations are zero, it could imply four independent processes or mechanisms underlying the performance on the four tests. Such evidence might also be used to argue against a single [illegible] system for processing information. Thus, although curiosity was a major reason for undertaking the initial studies, it was believed that the findings would not be without some theoretical interest and value, and this belief was strengthened as successive sets of data were examined.

Related Work

A consideration of the basic paradigm and the variations of it in which the subject was instructed to learn only some of the classes of materials shows that there was apparent contact with a number of phenomena and procedures described in the literature. We say apparent because in fact the contact is far less than firm. There is simply not a body of literature concerned with the learning of several tasks simultaneously. At a more abstract level, however, it is quite possible that our paradigm could produce phenomena that are empirically or conceptually related to previous work, even though most of the previous work has not dealt with memory phenomenon, and most of it has evolved from studies of perception in relatively simple situations. Some of the concepts associated with this previous work will be mentioned.

When our subjects were asked to learn fewer than the four tasks, although presented all four, selective perception, selection attention, or divided attention may be involved. When a subject attempts to learn several tasks simultaneously, many extant concepts are suggested: Parallel versus serial processing; single versus multiple channels; "space" limitations, or central-processing limitations; competition for processing capacity and subsequent interference; memory overload and filtering; trade-off relationships. Thus, it appeared possible that we had available a ready-made vocabulary if we chose to cast our findings into a framework established by research in which the emphasis was on perceptual processes, and where memory pehnomena, if considered, were of the very short-term variety. The nature of the interests which led to our inquiry recommended that we let our data guide us on such matters, although it was beyond doubt that certain of our predilections would play a role in the eventual choice of both descriptive and explanatory concepts.

It was stated above that there is not a body of research dealing with the simultaneous acquisition of two or more tasks. In one sense, this statement is false. We may believe that some of our frequently used laboratory tasks actually consist of two or more subsidiary tasks which are acquired simultaneously. Thus, we may view the paired-associate task as requiring the acquisition of response terms, and further requiring the acquisition of associations between the stimulus and response terms. Viewed across items in the task, both subsidiary tasks are being learned simultaneously. We presumably are able to study the acquisition of these two subsidiary tasks independently, as by using free recall to simulate response-term learning, and associative matching to simulate acquisition of associations. The difference between this approach and the one used in the present studies is that in the present approach we could study response learning and associative matching when different materials constituted each task, and when both tasks were presented for simultaneous acquisition.

Overall Plan

The central condition of the research was outlined above: The subjects were given four tasks to learn simultaneously. For reasons which will be explained at a later point, this central condition was called Condition FORD. There were two classes of parametric manipulations which were carried out as a means of elucidating the phenomena produced by Condition FORD. These two classes of manipulations were designed to reduce the amount of information the subjects intentionally processed. The first class was represented by seven conditions in which the subjects were instructed to learn less than all four tasks, although the material presented was exactly the same as that presented under Condition FORD. The second class consisted of 13 conditions in which the number of tasks represented in the learning material varied. The difference between these two classes may be illustrated. In the first class a condition was included in which the subjects were instructed to learn only the company names and the frequency with which state names occurred. However, the slides also included the traffic and points-of-interest signs, and street names. In the second class of manipulations, a condition was included in which the subjects learned the company names *and frequencies of state names*, but the slides did not include the material for the other two tasks. At a gross level, the comparison of the results across the conditions in the two classes produced conclusions concerning the effect of irrelevant material on intentional learning. And, memory tests for the irrelevant material in the first class of conditions provided measures of incidental learning.

The data-collection phase for the parametric conditions extended over two school years. The subjects were college students. The data for the first class of manipulations were collected during the 1974-75 school year, those for the second class during 1975-76. It is always a haunting possibility that a population shift may occur from year to year, or even from academic quarter to quarter. As a check on this possibility, the FORD condition was included three times during the two years. The results for the three replications of Condition *FORD* were quite comparable. Therefore, it seems justified to present the results for the 20 conditions in the two classes of manipulations as if they had all been collected simultaneously.

We may now look at the plan for describing the research. First, we will present the methods and materials used for Condition FORD, followed by an examination of the pehnomena which emerge as a subject memorizes four tasks simultaneously. Second, the results of the 20 parametric conditions will be detailed. These results led us to further studies of a more theoretical nature, and the evaluation of these studies will be the third step in the plan. Included in this section will be some data that we actually collected in the initial stages of the research as we asked about the effect of study time and the effect of the memory tests on subsequent performance. Finally, as a fourth step, we will summarize our findings.

THE BASIC PHENOMENA

The materials and methods used in the central condition (Condition FORD) evolved from an extensive pilot study, and from certain other manipulations devised primarily to examine the effect of study time and the effect of test trials on performance. No purpose will be served in detailing the procedures and results of the pilot study. The results for the conditions dealing with the effects of time and test trials are of interest, but will be best evaluated at a later stage when study time is the major variable. Our intent for the present is to describe the phenomena which emerged as the subjects attempted to learn four tasks simultaneously.

Method

Materials

The acronym FORD was used to represent the four tasks, with each of the four letters signifying a different task:

Task F: Frequency judgments, indicating the number of times each of several state names occurred on the slides.

Task O: Judgments of the order in which the streets were crossed in the imaginary drive.

Task R: Recall of the two-word company names.

Task D: Recognition or discrimination of traffic signs shown on slides from those not shown on slides.

The materials used for each of these tasks will now be described. The words representing the four tasks occurred on 24 successive slides. As may be seen in Figure 1, more than one class of stimuli occurred on each slide. There was also a primacy slide and a recency slide, but the materials on them never entered into the results.

Frequency. Ten state names were chosen for the frequency-judgment task: Vermont, Kansas, Maine, Utah, Iowa, Texas, Ohio, Oregon, Idaho, Georgia. In addition, two state names (Florida and Michigan) were used as zero-frequency or new items on all tests of frequency assimilation. The frequencies of occurrence of the state names were 1, 3, 6, 10, or 15, each frequency being represented twice. A given state name never occurred more than once on a slide.

Order. The seven street names used were Granite Avenue, Central Road, Hawthorne Lane, Compton Boulevard, Mission Street, Forest Court, and Mohawk Drive. These street names always occurred on slides 2, 6, 10, 14, 17, 20, and 23.

Recall. The material to be recalled consisted of 30 two-word names of fictitious companies. These 30 names are listed alphabetically in Table 1. The first word for each may be considered a trade name, the second word a product. Each name was presented twice and two

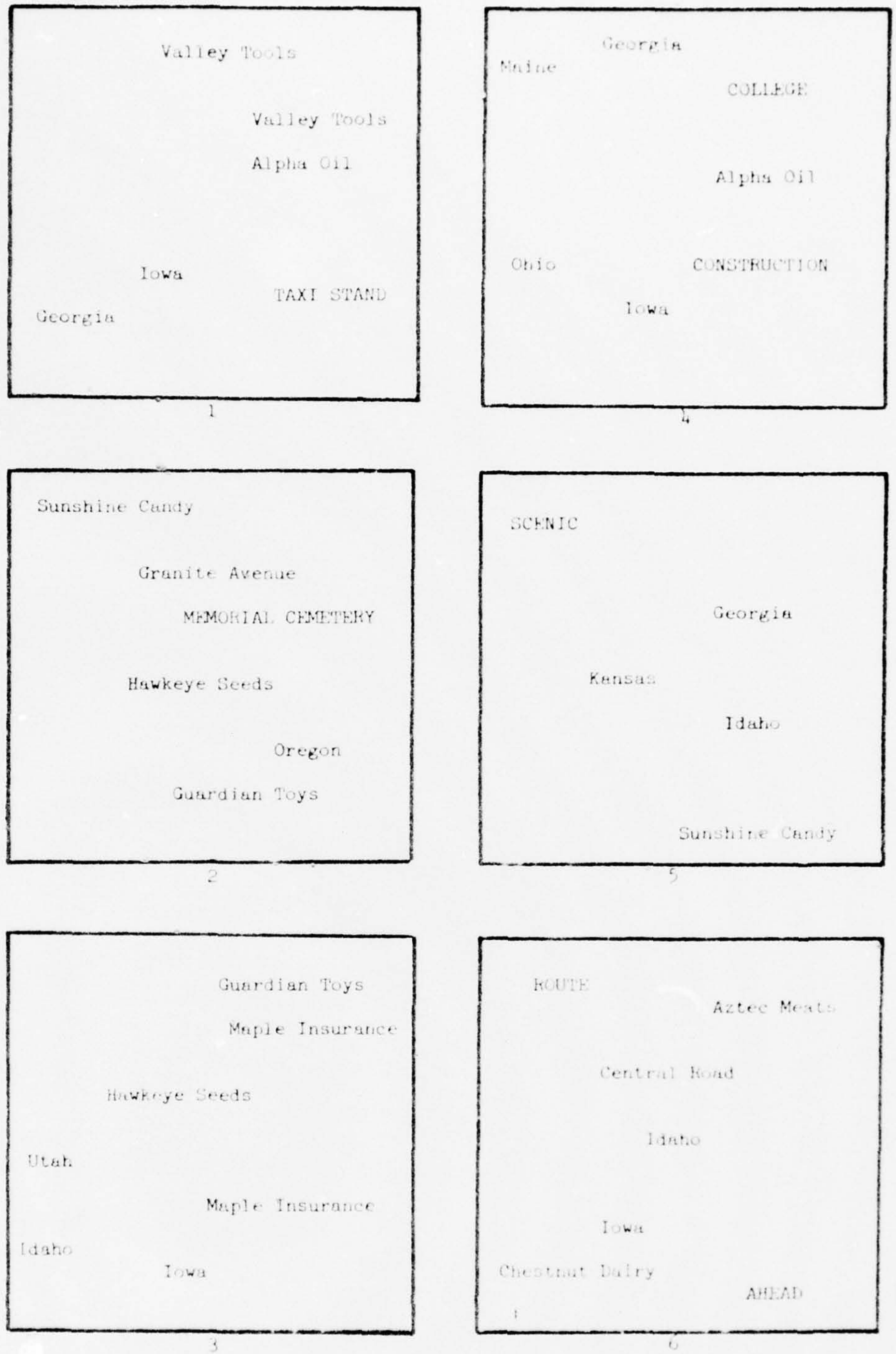

Figure 1. The first six slides used in Condition FORD. The state names were used for frequency judgments. Street names (Granite Avenue and Central Road, slides 2 and 6) were used to measure order information. The two-word company names were recalled, and the two occurrences of each name may have had spacings of 0 (Valley Tools, slide 1), 1 (Hawkeye Seeds, slides 2 and 3), or 3 (Alpha Oil, slides 1 and 4). Traffic signs and points-of-interest signs were always printed in capital letters, and were used in the recognition tests. These signs were presented as intact signs of two words (TAXI STAND, slide 1), or broken signs (SCENIC ROUTE, slides 5 and 6, or CONSTRUCTION AHEAD, slides 4 and 6).

Table 1

Company Names for the Recall Test

Acme Freight	Manor Flour
Alpha Oil	Maple Insurance
Anchor Glass	Merchant Distillery
Aztec Meats	Midwest Lumber
Bulldog Paper	Neptune Electronics
Chestnut Dairy	Prairie Books
Crystal Furniture	Rainbow Shoes
Eagle Chemicals	Ranger Steel
Guardian Toys	Spartan Realty
Hawkeye Seeds	Sunshine Candy
Ideal Motors	Supreme Apparel
Keystone Containers	Union Cement
Kings Tobacco	Valley Tools
Lakeside Paints	Victor Carpets
Magnum Cosmetics	Western Tires

variables were introduced into this repetition. One of these variables will be spoken of as the spacing variable, although it differs somewhat from the usual spacing manipulation. The three levels of spacing will be identified as Condition 0, 1, and 3. In Condition 0, the two occurrences of the name were on the same slide. For Condition 1, the two occurrences were on adjacent slides, and for Condition 3, the second occurrence was on the third slide after the slide of first occurrence. For example, if Eagle Chemicals first occurred on slide 8, the second occurrence was on slide 11 for Condition 3. With 30 names, 10 each were assigned to Conditions 0, 1, and 3. It was possible to arrange the items so that two items under each condition occurred in each fifth of the series of 24 slides.

A second variable in recall involved same and different cells in the repetition of items. In constructing the slides, we viewed each as consisting of nine cells in a 3 x 3 matrix. For Condition 0, which involved massed items, the two occurrences of a company name occupied adjacent cells on the slide (Condition Same), or were separated by a cell (Condition Different) which usually contained words for the other tasks. For Conditions 1 and 3, the two occurrences occupied the Same cell, or they occupied Different cells. Thus, the 10 names for each condition (0, 1, and 3) involved 5 names occupying the same cells on the slides (or adjacent, for Condition 0), and 5 names occupying different cells. Each of the unique six conditions (three levels of spacing and Same-Different) was represented by five items, with one item occurring in each successive fifth of the list. The Same-Different variable was viewed as an adjunct to the spacing variable. In the studies of spacing, the Same-Different variable has not received attention and we had no expectations concerning its influence.

Recognition. There were 32, two-word road signs used in the recognition tasks. These are listed in Table 2. Twelve of these signs were presented as intact pairs of words and these will be considered as old items on the recognition test. Eight of the items were not presented on the slides but occurred as new items on the recognition test. The remaining 12 signs were used to implement what we will call the separation variable. For these 12 road signs, the two words were presented on separate slides. Thus, EXIT might have been presented on one slide, RAMP on a subsequent slide. On the test, the subjects were presented only two-word signs and they had to decide whether both words had or had not been presented on the same slide. When the two words of a sign were presented on separate slides, they always occupied the same cell on the two slides. However, the amount of separation of the two words was systematically varied. More specifically, there were three degrees of separation identified as separations 1, 2, and 4, where the number refers to the number of slides following the appearance of the first word that the second word occurred. Thus, for Separation 1, the two words were on adjacent slides; for Separation 2, the two words were

Table 2

Traffic and Point-of-Interest Signs for Recognition

AFTON RIVER	INDUSTRIAL PARK
ART FAIR	KEY AIRPORT
BARGE CANAL	LOADING DOCK
BIRD SANCTUARY	LOGAN SCHOOL
BOAT LAUNCH	MEMORIAL CEMETERY
CHILDREN PLAYING	MERCY HOSPITAL
CITY LIMITS	MUNICIPAL ZOO
CIVIC PLAZA	PEDESTRIAN MALL
COLLEGE STADIUM	RESIDENT PARKING
CONSTRUCTION AHEAD	REST HOME
COUNTRY CLUB	SCENIC ROUTE
COUNTY BUILDING	STATE POLICE
EMERGENCY PHONE	STRAY DEER
EXIT RAMP	TAXI STAND
FIRE STATION	TRAIN CROSSING
HISTORICAL LANDMARK	VILLAGE LIBRARY

separated by one slide, and for Separation 4, by three slides. The order of the two words was always proper; that is, RAMP never preceded EXIT in the study list.

Having described the materials for each of the tasks, it will be well to transmit some feeling for the tasks as they were presented simultaneously to the subject. The first six slides for one of the forms are seen in Figure 1. We have spoken of nine cells for each slide. Actually, as can be seen, we attempted to minimize the likelihood that the subject would view the slides as having nine cells. This was done by positioning items within a cell on a random basis.

If each slide is considered to have 9 cells, there were 216 cells for the 24 slides. Of these, 173 were filled, 43 were vacant. The filled cells required 70 for state names, 7 for street names, 60 for company names, and 36 for traffic signs (12 for old items, 24 for the 12 signs used for the separation variable).

Forms. The independent variables in Condition FORD were manipulated within subjects. Particular items were given particular functions. In order to eliminate the confounding between items and functions, three forms were constructed. The function served by an item was determined randomly for each form, subject to the restriction that the item was not allowed to serve the same function in more than one form. Some consequences of these procedures will be mentioned for each task.

For frequency judgments, 10 state names were used. There was only one independent variable, frequency of presentation. Across the three forms, therefore, a given frequency level was represented by six different items.

The seven street names were simply ordered three times, with the same street not allowed to occupy a given position in the series more than once.

For recall, each level of the spacing variable was represented by 10 items on each form, hence 30 different items across the three forms. When both the spacing variable and the Same-Different variable are considered, 15 different items were represented across the three forms, e.g., with Condition 1 for spacing, and Same with regard to spatial position on the two slides, 15 different items were used across the three forms.

For recognition, there were three basic types of items, namely, old (12 items), new (8 items) and separated (12 items). In constructing the three forms, assignment to function was random subject to the restriction that an item could not serve the same basic function more than twice, and the number serving the same function twice was kept at a minimum. Across the three forms, each level of the separation variable was represented by 12 different items.

The purpose of using three forms was to minimize the likelihood that differences in item difficulty would bias the results. We have no interest in forms per se and our analyses have ignored forms as a statistical factor.

Procedure and Subjects

Design. Subjects were tested in groups in a room used only for group experimentation. Condition FORD, with which we are presently concerned, involved 126 subjects. This total was made up of three groups of 54, 36, and 36 subjects tested at different times over a two-year period. In each case, other conditions were included. The unit for group testing was six subjects. Conditions and forms within conditions were block randomized with six subjects being the unit. For example, when Condition FORD was made up of 54 subjects, two other conditions of 54 subjects each were also included, the results for which will be reported later. With three conditions and three forms, 27 units of six subjects each appeared in the randomized schedule. Thus, for a given condition and form, three different groups of six subjects each were tested. When 36 subjects were tested in a condition, the randomized schedule for testing involved two groups of six subjects each for a given form within a condition.

As noted, the group unit was six subjects. As would be expected, in many cases all six subjects scheduled for a given period failed to appear. A single method was used for handling this matter throughout all of the experimentation. All sessions as given on the random schedule of conditions were carried out in order as if all six subjects had appeared for each session. After the completion of these sessions, we "started over", scheduling new sessions for those where less than six subjects had appeared, the number of subjects scheduled being the number necessary to equal six, including those tested originally. If necessary, as was usually the case, this procedure was repeated a third time so that all groups as included on the original schedule sheet were represented by six subjects.

A further breakdown was necessary in the design. It will be remembered that we were interested in the permanence of the memories established. To vary the retention interval, we varied systematically the order of testing of the tasks. There were three test sheets, one for recall, one for recognition, and one which included the tests for both frequency estimation and street ordering. There are six possible ways in which the three test sheets can be ordered; each of these orders was used equally often for each form within each condition. It was possible, therefore, to examine performance on each task when it was tested first, tested second, and tested third in the series of tests. The retention interval for a given task was, of course, filled by tests being given on the other tasks.

Instructions and tests. For Condition FORD, the initial instructions were complete with regard to the material to appear on the slides, and with the methods for testing each of the four classes of materials. These instructions will be quoted in their entirety:

> I am going to present to you a series of slides which will be projected on this screen. Each slide will contain a number of words. Some items will be single words, and other items will be two-word units. They will all be common words. In viewing each slide, I want you to imagine that you are taking a ride in a car. The items that you see on each slide might be those that you would actually see when you glance out the car window every so often during this imaginary drive. As is true when taking a drive, you will see some words more than once. At

the same time, of course, new words will keep appearing as the car moves through the streets. Your task is to view each slide carefully and try to remember as many items as you can. You will have 20 seconds to do this for each slide. Of course, I am going to test your memory for the events you saw during this imaginary ride, and I will now tell you exactly how you will be tested.

On the slides are the names of a number of the states of the union. You may think of these as representing automobiles with different state license plates. A list of states will be given on the test. You will be asked to estimate the number of times you saw cars from each of the states. We will not be dealing with all 50 states, but we will be dealing with a number of them. You will not be able to count the number of times you see a given state plate. Rather, try to get an impression of the number of times you see a state name so that you can give an estimate of the number of times you saw license plates for each state.

Among the things you might see on the drive would be a number of signs for commercial companies. On the slides, these are all two-word signs, and each includes the product's name. For example, you might see a sign for "Hamm's Beer", although the signs you will actually see are fictitious. On the test, you will be asked to recall and write down as many of these company names as you can remember.

Included on the slides are two-word traffic signs and two-word point of interest signs. BUS STOP and RACE TRACK are two examples of these. These are always printed in capital letters so that they are distinguishable from the other materials. Sometimes the two words appear on the same slide. At other times, the two words might appear on different slides. That is, you might see BUS on one slide and STOP on the next slide. To take another example, RACE might be on one slide and TRACK might appear two slides later. On the memory test, you will be asked to identify which two-word traffic signs and two-word point of interest signs were printed on the same slide and which were printed on different slides (that is, those signs which had one of the words on one slide and the other word on a different slide). Also, some items will be given on the test which did not appear on any slide; you will be asked to indicate these. You will be given, therefore, a recognition test for those traffic signs and point of interest signs printed in capital letters.

Finally, we will occasionally cross a street as we take this ride. You will know this because the name of the street will be printed on the slide. In fact, seven different streets will be crossed. On the test, you will be given the seven streets, and you will be asked to indicate which street was crossed first, which street was crossed second, and so on. Thus, you will be tested for the order in which the streets appeared.

Following these instructions, the experimenter answered any questions concerning the procedure and testing. Then, the 26 slides were presented, involving the 24 experimental slides and a primacy and recency slide. The slides were projected by a Kodak Carousel, and each slide was presented for 20 seconds as controlled by a peripheral timer. This interval was chosen after some pilot work and we will have more to say about the effect of exposure duration at a later point.

After the last slide was presented, test booklets were distributed. The face sheet contained instructions which were read by the experimenter as the subjects followed. The instructions were:

> After the experimenter tells you to turn this page, you will begin working on several different memory tests. You <u>must</u> take the tests in the order in which they are given in this booklet. Do <u>not</u> take the tests in any other order. You <u>must</u> complete work on a given test before going on to the next. After completing a test, you can<u>not</u> go back to previous ones. Instructions for each test are on the test page. Read them carefully--if you have any questions, raise your hand. Some tests may require you to answer <u>all</u> items, guessing if necessary. If this is so, please be sure to give a response to all items.

The instructions for each test were as follows:

> <u>Frequency</u>: The names of a number of states appeared on the slides, representing the number of automobiles from each state which were seen during the drive. You are to estimate the number of cars from each state which you think you saw. The states are listed alphabetically, and after each is a blank. Put a number in each blank to indicate your estimate of the number of

cars seen from that state. If a state name is given that you think you didn't see at all, put a zero in the blank. You must have an entry for each blank.

Order: During the drive, seven streets were "crossed", the name of each of the seven appearing on a slide. The seven names are listed below alphabetically. You are to indicate the order in which the streets were crossed. Put a 1 after the street that you think was crossed first during the drive, a 2 after the street you think was crossed second during the drive, and so on, assigning a 7 to the street crossed just before the end of the drive. You must use each number only once; each blank must contain one of the numbers.

Recall. There were 30 fictitious company names given on the slides. All consisted of two words, with the second word indicating the product involved. One example of this might be "Hamm's Beer", but of course all names used were fictitious. Write down as many of these two-word company names as you can. You may write them down in any order you wish. Do not linger too long in trying to remember the names. No one would be expected to remember all 30 names, but "dredge up" all that you can within a reasonable amount of time before moving on.

Recognition: A number of two-word traffic signs and two words indicating points of interest appeared on the slides. These were always printed in capital letters. These are listed below alphabetically along with some two-word signs which did not appear on any slide. For each, you are to make a YES or

NO decision. If the two words did not appear at all, you are to encircle NO. Furthermore, if the two words did not appear on the <u>same</u> slide, you are to encircle NO. You will circle YES only if the two words appeared <u>together</u> on a slide. <u>You must make a decision for each, guessing if necessary</u>.

At the bottom of each of the three test pages a blank occurred with the word "time" under it. When each subject completed work on a test page, he wrote the elapsed time in the blank. Therefore, a record was available of the time it took each subject to complete each test page.

When all subjects had completed the tests, and the booklets had been collected, the experimenter said to the subjects:

> We are now going to run through the same set of slides again. The slides will be in exactly the same order as before. Once again, I want you to study each slide carefully, remembering as much information as possible. After the slide presentation, you will be tested again in <u>exactly</u> the same way. I want you to be ready for the test so I will hand out the test booklets now. Print your name on them as you did the first time. Remember not to begin working after we see the slides until I give you the signal.

The slides were then presented for the second time, followed by the tests which, as the subjects were informed, were exactly the same as given for the first trial.

Results

In presenting the results for Condition FORD, the performance on each of the four tasks will be considered in order, followed by an examination of the correlational evidence reflecting the relationships among the tasks. We will, of course, include statistical analyses where it seems necessary, but we have made a definite attempt to avoid spending time on small differences or small effects unless they seemed to be of particular theoretical importance. In most cases, the data shown in graphical form provide presumtive evidence for reliability.

Frequency

State names were judged as to the number of occurrences across the 24 slides. The true frequencies were 1, 3, 6, 10, and 15. The results are shown in Figure 2, where the mean judgments of frequency are related to true frequency. Apparent frequency increased directly as true frequency increased, and, as commonly found, low frequencies were overestimated, high frequencies were underestimated. The mean judgments on Trial 2 were slightly higher than on Trial 1 at all five frequency levels. It might be presumed that this small but constant increase in judged frequency between Trial 1 and Trial 2 represented an increment produced by the state names each occurring once on the test sheets for Trial 1. If this is reasonable, there should also have been an increase for the two state names appearing on the test sheet but which had not appeared on the slides. For these two states, mean judged frequency was .70 for Trial 1 and 1.19 for Trial 2, thus supporting the idea that the act of testing increased apparent frequency.

An unexpected finding was that the precision of the frequency

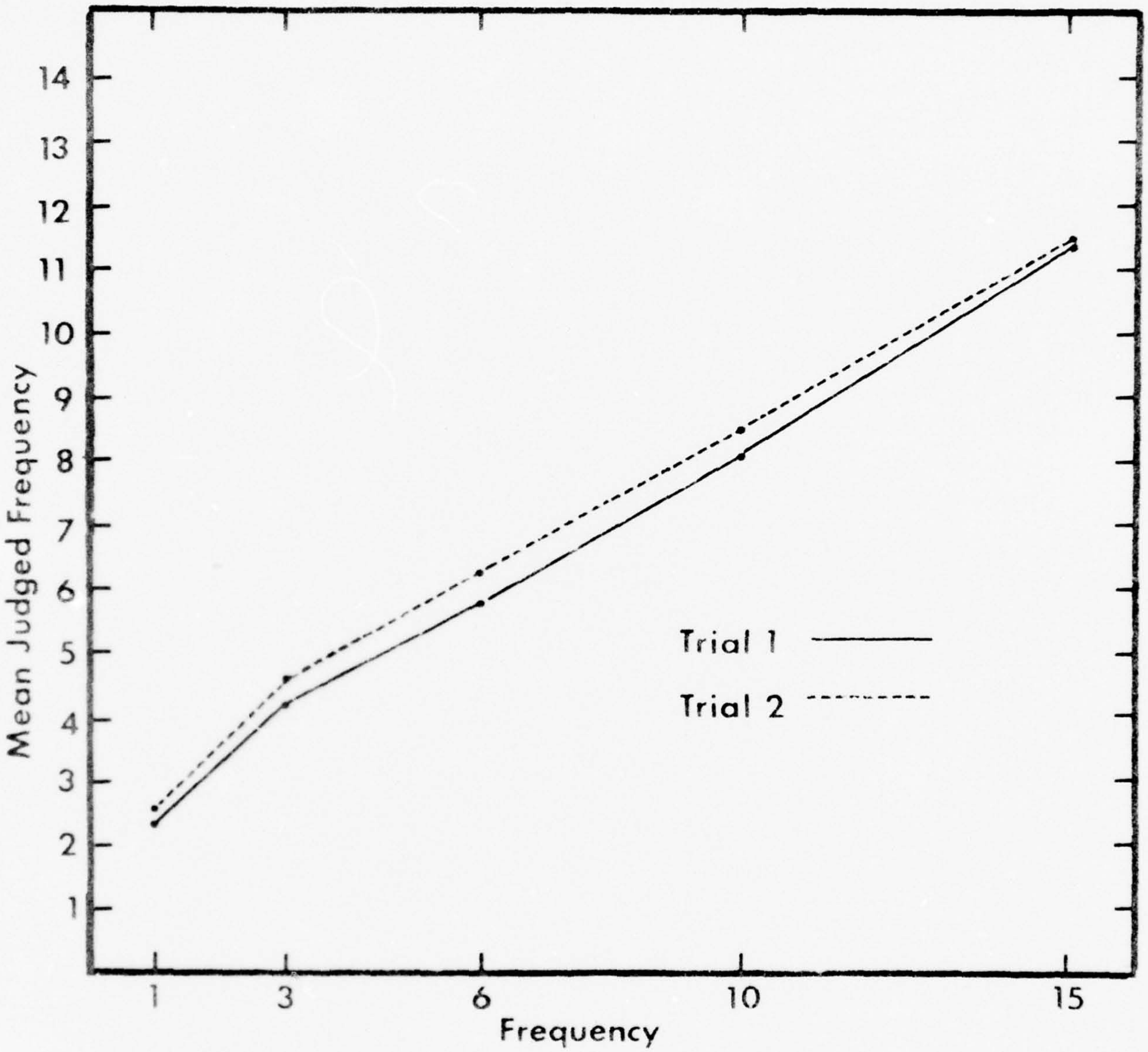

Figure 2. Mean frequency judgments for Condition FORD as a function of true frequency and trial.

judgments did not increase from Trial 1 to Trial 2, a finding which is inferred by the lack of interaction between frequency and trials in Figure 2. We will return to this matter shortly.

In experiments in which frequency judgments represent the only task given the subject, loss of frequency information over time occurs very slowly (e.g., Underwood, Zimmerman, & Freund, 1971). Still, even with the relatively short retention intervals involved in the present study, some loss may occur because the intervals were filled by the subjects being engaged in taking other tests of memory. We will examine the frequency judgments as a function of the length of the retention interval. The procedure used needs to be given in some detail.

It will be remembered that the subjects recorded the elapsed time as they completed each of the three test sheets, so it was possible to specify three retention intervals, with the performance on any given task at each of the three retention intervals being represented by different groups of subjects. In Condition FORD, 42 subjects were represented at each interval. We will identify the three intervals as short, medium, and long, but it is necessary to provide rough evidence on the length of the intervals.

On the first test trial, the short interval (time to first test) consisted of 20 seconds for the recency slide, plus 40 to 60 seconds required to distribute the booklets and give the initial test instructions. Thus, the short retention interval (the time between the presentation of the last experimental slide and the beginning of the first test) was something in the neighborhood of 1 minute. The medium and long intervals were determined by the average time required to complete the first and second memory tests, respectively. In the present case we are interested in the frequency-judgment tests, so the medium and long intervals were produced by the time required to complete the recall and recognition tests. The average medium and long retention intervals for frequency judgments for Trial 1 were 2.09 and 5.42 minutes from the point in time that the subject started the first test.

On the second test trial, the booklets were immediately available to the subjects, and no instructions were necessary. Furthermore, the data indicate that the subjects required less time to take the tests on the second trial than on the first. The medium and long retention intervals for the second test trial were 1.70 and 4.23 minutes.

Loss of frequency information over time would be indicated by flattening of the line representing the relationship between true frequency and judged frequency. This could be determined directly from the raw frequency judgments. However, for other purposes we wanted measures of each individual's performance relating true slope and judged slope. Therefore, for each subject we calculated a product-moment correlation between the true frequencies and the judged frequencies of the 10 state names. These correlations do not reflect differences among individuals in level of absolute judgments but they should reflect accurately differences in the degree to which the judged slope approximated the true slope. For statistical purposes, each r was transformed to z', although for graphing and expository purposes, we have retransformed the mean z' to r.

For Trial 1, the mean correlations for the short, medium, and long retention intervals were .81, .79, and .83 respectively. The corresponding values for Trial 2 were .79, .78, and .81. None of the differences was reliable satistically. These data show, as did those used to plot Figure 2, that performance in estimating frequency of events did not improve from Trial 1 to Trial 2, and that this performance did not change over the retention intervals. The lack of increase in precision from Trial 1 to Trial 2 might be attributed to a ceiling effect; that is, in an absolute sense performance was quite good on the first trial, and it may be that any improvement beyond that would be difficult to obtain under any circumstances. However, in other conditions in which there were fewer tasks (to be described later), the subjects were able to improve their performance from Trial 1 to Trial 2 even when performance was better on the first trial than it was for the present condition. For the time being, therefore, we must consider the possibility that the lack of improvement from Trial 1 to Trial 2 in frequency judgments resulted in some way from the demands placed on memory by the other tasks.

Order

In this task the subjects were required to identify the order in which the seven streets were "crossed." The task may be viewed as a serial learning task without the necessity of recalling the street names. As a first response measure we have used the mean position judgments of the seven streets. These are shown in Figure 3. The clear slope for Trial 1 indicates that some serial order information was available, and the obvious increase in the slope from Trial 1 to Trial 2 shows that additional learning occurred on the second study trial. We also examined the number of hits (assigning correct numbers) for each street. These data showed that the number of hits was greater for the first and seventh streets than for the other five, but there was little difference among the other five. The number of hits increased for all positions between Trial 1 and Trial 2.

To examine the effect of the retention interval, we have calculated the r relating true and estimated position of the seven streets for each subject. Because the tests for frequency and for order were on the same test sheets, the retention intervals were approximately those given earlier for the frequency judgments. As would be expected from the evidence in Figure 3, the mean r increased substantially between Trial 1 (.47) and Trial 2 (.89). On Trial 1 the mean correlations for the short, medium, and long retention intervals were .28, .60, and .50. For Trial 2, the corresponding values were .91, .86, and .89. Overall, the interval effect was not reliable statistically, but there was a significant interaction (p = .01) between interval and trial. This effect is due largely to the low correlation on the first trial for the short retention interval. We have not been able to give a reasonable explanation of this finding, although these data are clean in showing that performance did not deteriorate as the retention interval increased.

Recall

We counted as correct only those items in which both words identi-

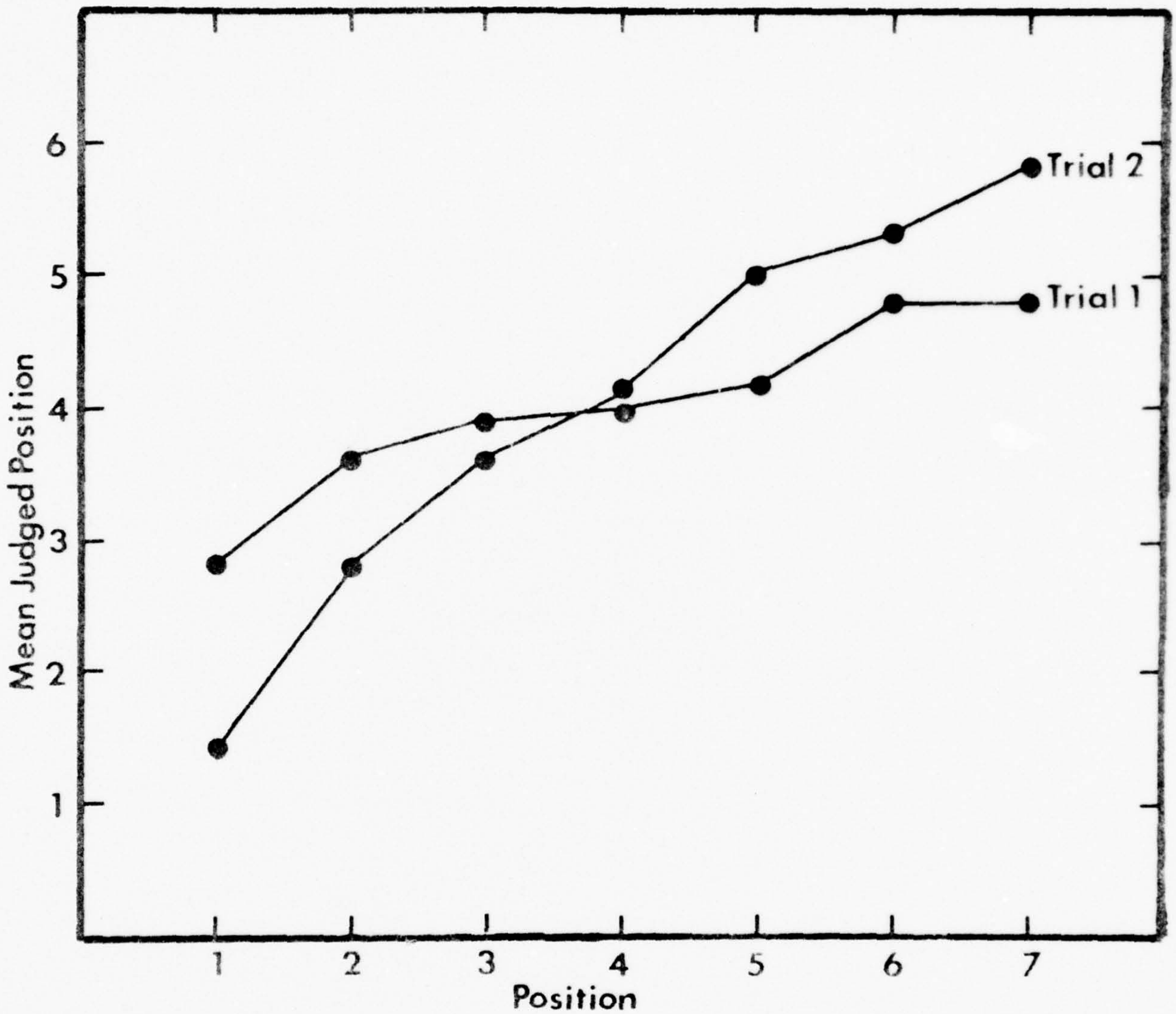

Figure 3. Mean order (position) judgments for Condition FORD as a function of true position and trial.

fying a company were recalled together. The results for each trial are shown in Figure 4, with the spacing variable on the baseline, and percent recall on the ordinate. As is apparent, in an absolute sense performance was very poor on Trial 1. The interaction between the spacing variable and trials is quite evident ($\underline{p}$ = .001). The difference on Trial 2 between recall with a spacing of 0 and recall with spacings of 1 and 3 is large. This is true in spite of the fact that the so-called massed practice (0 spacing) becomes spaced practice when viewed across the two trials. Recall with a spacing of 3 was obviously not better than recall with a spacing of 1, although as will be seen later, this finding did not always hold when the number of tasks was reduced.

It will be remembered that a Same-Different variable was included in the positioning of the company names. The two occurrences of the name were in the same cell on both slides or in different cells on the two slides. This variable had no effect overall ($\underline{p}$ = .26), but it did interact with the spacing variable ($\underline{p}$ = .004). This interaction is seen in Figure 5, for Trial 2. The data indicate that when the two occurrences were on successive slides (Condition 1), performance was better if the same cells were used to show the company name than if different cells were used, whereas with spacings of 0 and 3, there was a slightly opposite effect. None of the other interactions was reliable.

We turn next to the influence of the retention interval. The length of the retention intervals were calculated in the same way as was done for the frequency judgments. For Trial 1, the medium and long retention intervals were 1.78 and 3.85 minutes, respectively. For the second trial, the corresponding values were .90 and 1.99 minutes. Again, these values represent the time which elapsed from the moment the subjects actually started to take the first test. It may be observed that these intervals were shorter than were those found for the frequency-judging task. This results from the fact that subjects in general spent more time on the recall test than on the other two test sheets.

There was no effect of the retention interval on recall. For the short, medium, and long retention intervals, the recall values for Trial 1 were 7.9, 8.3, and 7.1%, respectively. For Trial 2, the values were 29.8, 31.4, and 31.0%.

Effects of position. There are still further questions to be asked about the retention interval and recall. It will be remembered that the second occurrences of the 30 company names were arranged so that six occurred in each fifth of the list. We may, therefore, ask about recall as a function of both position and interval. The results of this analysis are shown in Figure 6, for each trial. On Trial 1, there appears to be a small recency effect at the short interval; this is true in spite of the fact that there was a recency buffer slide, and approximately one minute was involved in giving instructions

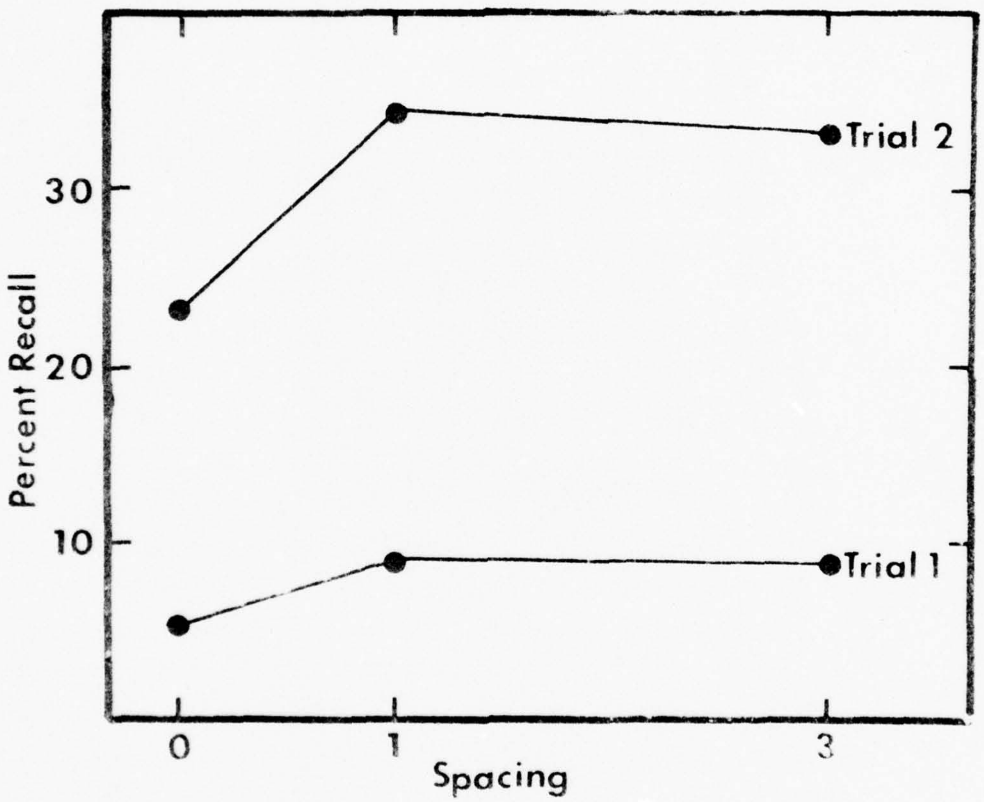

Figure 4. Recall for Condition FORD as a function of spacing and trial.

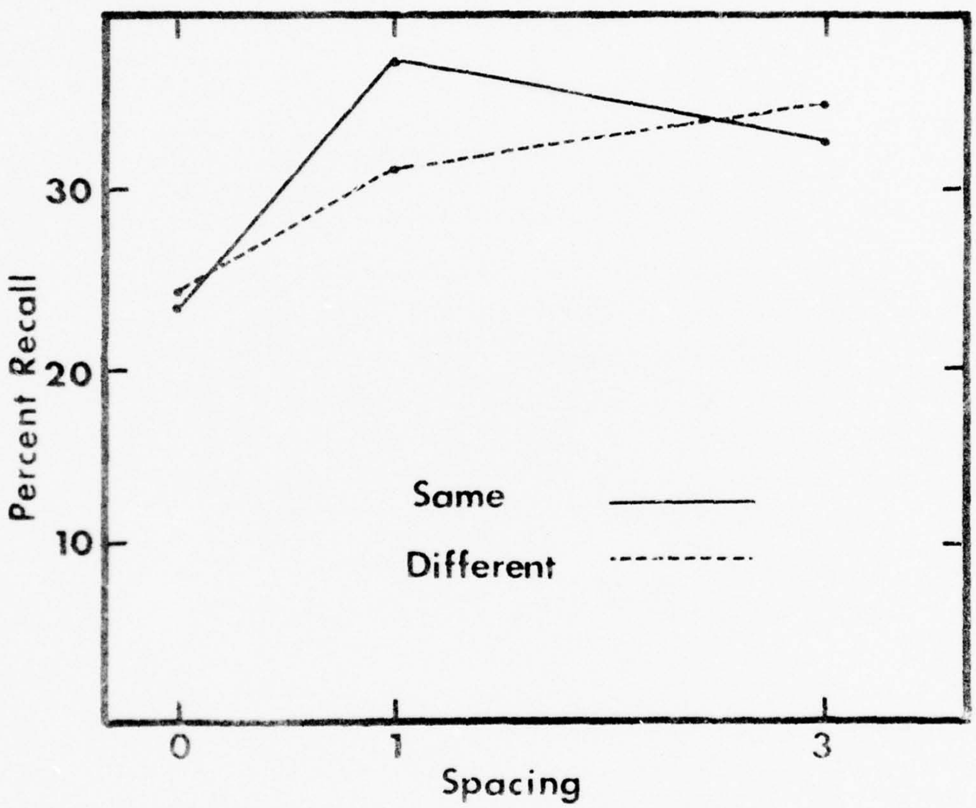

Figure 5. Recall on Trial 2 for Condition FORD as a function of space and the Same-Different variable.

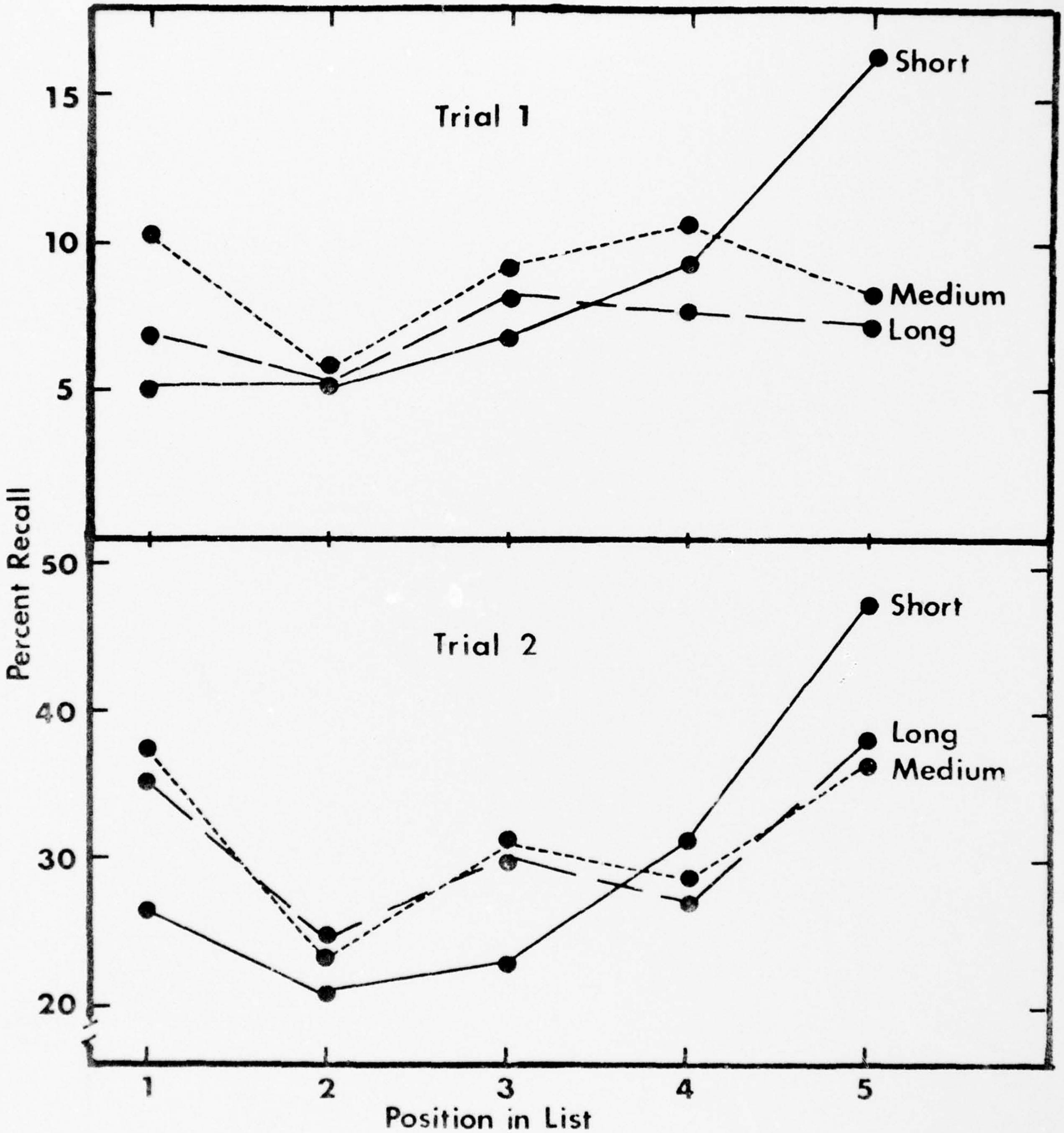

Figure 6. Recall in Condition FORD as a function of position in the list (1 through 5), length of retention interval (short, medium, long), and trial.

and distributing booklets before recall was initiated. This recency-like effect becomes more apparent on Trial 2, where a small primacy effect also appeared for all intervals. There appears to be a compensatory effect in that recall was better for the early positions with the longer retention intervals than it was with the short interval, whereas on the fifth position the reverse was true. For Trial 2, both the effects of position and the interaction between position and interval were reliable ($p < .01$). It is worth mentioning that in the large pilot study where names of existing companies were used, the same interaction between interval and position was also found.

We believe that the interaction shown in Figure 6 is a consequence of two factors. First, for the short retention interval, the items first recalled tended to be items from the later positions in the list. This was determined by making a tally of the study positions for the first half of the items recalled on Trial 2. This tally showed that 64.2% of the items appearing in the first half of the recall protocol came from positions in the second half of the study list. For the medium and long intervals, the values were 49.3% and 47.5%. When the bias at the short retention interval for recalling items positioned late in the list is viewed in conjunction with the recency effect, it might seem that the recency effect results from the production of items from a short-term store as recall is initiated. The difficulty with this interpretation is that recency effects also occurred on Trial 1 where at least a minute elapsed between the last experimental slide and the point in time at which the subject initiated recall for the short interval. It should also be noted that the subject did not know at that time that his first retention test was to involve recall.

The second fact that must be considered is the time spent by the subjects in attempting to recall. The time measures indicate that the subjects spent more time in attempting to recall after the medium and long retention intervals than after the short retention interval. For the three intervals in order the values were 2.59, 3.53, and 3.90 minutes. It appears, therefore, that subjects with the short retention interval spent less total time in trying to recall than did subjects with the longer intervals, and that they spent a disproportionate amount of this time trying to recall items occupying positions in the second half of the study list. It is not apparent to us why the attention should be directed toward items in the later positions for the short interval and not for the longer intervals.

Recognition

Misses and false alarms. We ask first about simple recognition expressed as the number of misses on the old signs plus the number of false alarms on the new signs. At the same time we may look at the effects of the retention intervals. The medium and long retention intervals for Trial 1 averaged 2.56 and 4.71 minutes in length, respectively, with the averages for Trial 2 being 1.80 and 4.01 minutes. The results, as seen in Figure 7, show a sharp reduction in

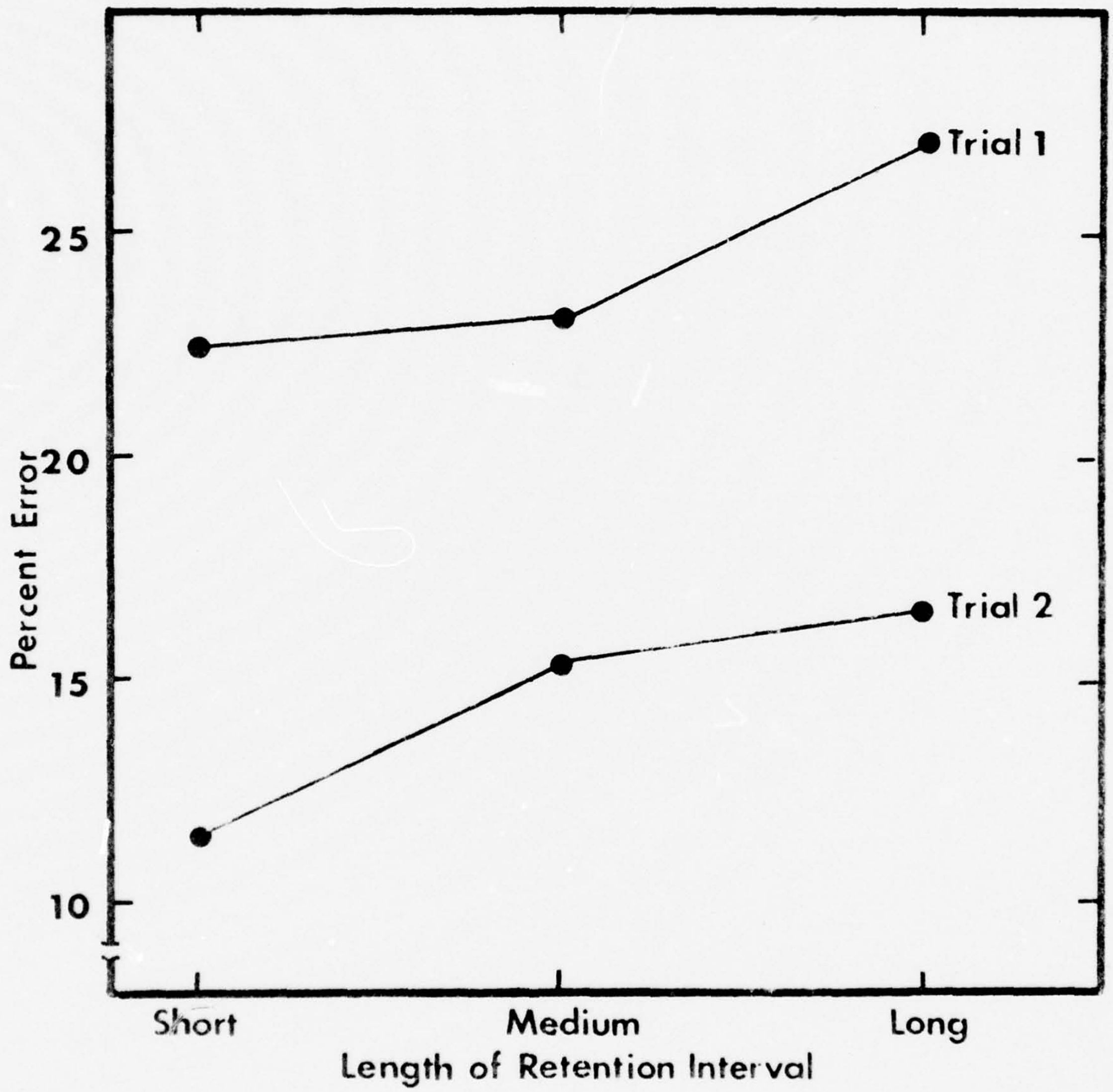

Figure 7. Recognition errors for Condition FCRD as related to length of retention interval and to trial.

the error percentages from Trial 1 to Trial 2, and some increase in errors as the retention interval increases. However, this latter effect is not reliable statistically (p = .08), and thus, for a fourth task we have again found that the memories established on the study trials were relatively stable.

Separation variable. Among the signs presented were 12 in which the two words for a given sign occurred on different slides. There were three levels of separation of the two words, namely, 1, 2, or 4 slides. On the test, all of the signs were given as two words and the subjects had to decide whether the signs had been presented on the slides as intact signs. The false alarms made to these 12 signs were compared with those made to new signs (control). Again, length of the retention interval did not influence the outcome reliably; therefore, the results as seen in Figure 8 are depicted in terms of separation and trial.

There can be no doubt that presenting the two words separately produced more false alarms on the test than were produced to new signs. "Inserting" the two words separately into the memory system influenced decisions about whether the words had or had not been presented together during study, and this was true for both trials. Yet, the distance by which the two words were separated on the study trial was of no consequence in determining the number of false alarms.

Interrelationship Within and Among Tasks

We know very little about subjects' "strategies" when they are given four tasks to learn simultaneously. Some subjects might, at one extreme, attend primarily to one of the tasks, essentially ignoring the other tasks. Or, a subject might attend to one or two of the tasks on the first study trial, and to the other tasks on the second study trial. Some preliminary evidence on such issues can be gotten from an examination of the correlations within and between tasks.

In calculating the correlations to be reported here, and in all studies to be reported later, we have used standard scores (x/σ) where the mean and standard deviation used for deriving a standard score were based on the scores within a form. For the present data, there were 42 subjects assigned to each form, and the mean and standard deviation of the 42 scores were used to obtain a standard score for each subject. The correlation was then calculated on all 126 subjects. In conditions to be reported later, we usually used 36 subjects in a condition, 12 being assigned to each form. In these conditions, just as in the present FORD condition, the standard score for each subject was based on the mean and standard deviation of the subjects within each form, although the correlation was calculated across the 36 subjects.

For frequency judgments and serial ordering, z' scores have been used to reflect individual performances. For recall, the individual

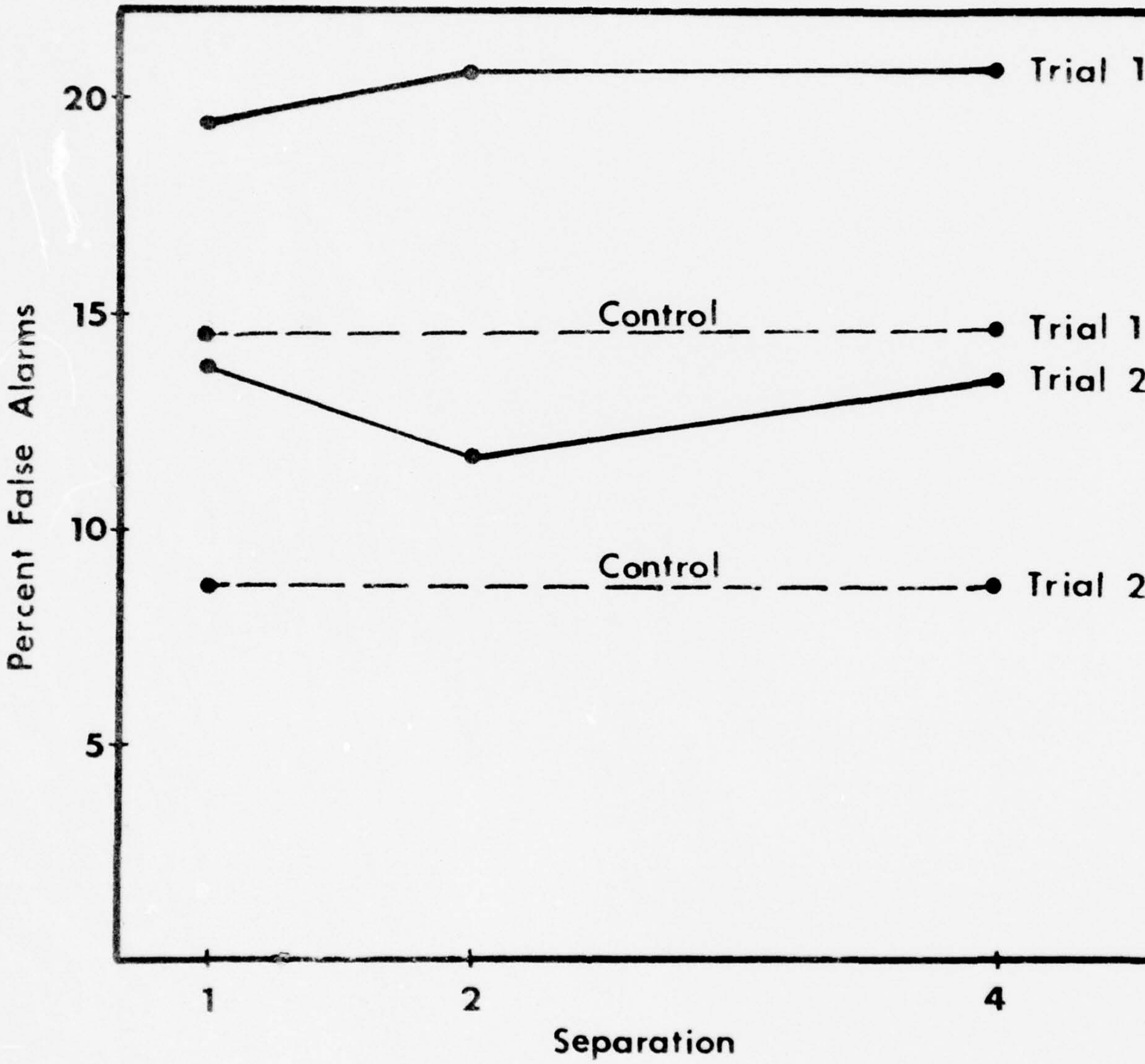

Figure 8. False alarms on separated items for Condition *FORD* as a function of level of separation and trial. The control levels were established by errors on new items.

scores were total items recalled, and for recognition we used the total errors (false alarms on both new and separated items, plus misses on old items).

To ask about the correlations within tasks is to ask about the reliability of performance, and for this purpose the correlations between performance on Trial 1 and Trial 2 were used. If these correlations are high, it can be inferred that subjects were probably not concentrating heavily on one or two tasks on the first study trial, and on the other tasks on the second study trial. If the subjects did do this, the correlations could not be high unless all subjects shifted in the same way, which seems quite unlikely. Substantial correlations between Trial 1 and Trial 2 would seem to imply some consistency in the distribution of effort on the two trials. With 126 subjects, a correlation of .23 carries a p value of .01.

For frequency judgments (Task F), the correlation between Trial 1 and Trial 2 was .62; for order (Task O), .30; for recall (Task R), .64; and for recognition (Task D), .56. We consider these correlations to be substantial for all of the tasks except Task O. Does the relatively low correlation for this task imply that subjects may have used this task as a pivotal trade-off task? We think not, and there are two reasons. First, it is clear from the data presented that the subjects as a whole did not ignore Task O on the second trial because there was a marked improvement in performance between Trial 1 and Trial 2. Second, other data (Underwood, 1977) suggest that as subjects attempt to identify the temporal order of events, their performance on initial learning trials is inherently unreliable. Because of this basic unreliability, we have not included this task in many of the manipulations to be presented later. We are led to the conclusion that the relatively low relationship between Trial 1 and Trial 2 performance for Task O cannot be easily interpreted as representing shifts in the subjects' attention from task to task.

The evidence on the frequency judgments (Task F) showed that performance did not improve from Trial 1 to Trial 2. This might suggest that the subjects as a group may have neglected this task on the second study trial. This could happen and still the data would show reliable performance from Trial 1 to Trial 2. At the extreme, we would simply be measuring the memories established on Trial 1 twice. This does not now seem to be the most appropriate interpretation. We will later present evidence which shows that even when the subject is given only Task F, the reliability is no higher than for the present data even though performance improves from Trial 1 to Trial 2.

We believe that for the three tasks (F, R, and D) the reliabilities are such as to indicate that the subjects had been reasonably consistent in allotting their learning time for the tasks across the two trials. Given this state of affairs, we may then ask about the intertask correlations as a means of estimating the commonality of processes underlying the tasks. We have used the totals across the two trials as the measure. The data show the following: Task F x Task R, .12; Task F x Task D, .30; and Task R x Task D, .35. Although the last two correlations are highly reliable statistically, the amount of shared variance is sufficiently small to require consideration of the possibility that in learning these tasks simultaneously there are fundamental differences in the processes underlying them.

Summary

The present central condition provided data that were relevant to three of the four purposes which led to the research. These purposes provide a convenient basis for organizing a summary of the results of Condition FORD.

One of the questions we asked concerned the universality of certain memory phenomena. These phenomena have been produced and studied primarily through the use of individual tasks; we asked whether or not the phenomena would be observed when the subjects learned four tasks simultaneously. Three phenomena were of particular interest.

1. The data indicated that relative frequencies of multiply-occurring events were assimilated with high fidelity during a single trial. And, as commonly observed in simple situations, events with low frequency were overestimated, those with high frequency were underestimated. We were at this point somewhat puzzled by the failure of the memory for frequency differences to become more precise as a consequence of the second study trial.

2. The recall data showed a phenomenon quite like the spacing effect which has been found in so many different situations (Hintzman, 1974). If a company name occurred twice on the same slide, it was less likely to be recalled than if it occurred once on each of two slides. There was no lag effect in that recall for items was as

high when the names were on adjacent slides as when two other slides fell between the two occurrences. Available studies using single tasks show that the level of lag beyond zero sometimes influences performance and sometimes does not, but the critical interacting variable(s) have not been clearly identified. Data to be presented later will have more to say about this matter, and also about the role of Same versus Different spatial positions, a variable which interacted with the degree of spacing.

3. The recognition data showed the "spontaneous integration" phenomenon for two-word road signs when the words had been presented separately, and, as demonstrated in the single-task situation (Underwood, Kapelak, & Malmi, 1976), the degree of separation had no influence on the probability of spontaneous integration.

Generally speaking, the results have shown that phenomena occurring in the single-task situation will not disappear or change in any dramatic manner when the subject is required to learn four tasks simultaneously.

A second question concerned the stability of the memories established under the flooded conditions. We did not find a reliable loss for any of the memories up to several minutes. There are three basic facts which must be born in mind when evaluating the lack of forgetting. First, no retention interval was less than 20 seconds (time of presentation of the recency buffer slide). This short interval occurred only on the second test trial; for the first test trial, the shortest interval was at least 60 seconds. Therefore, if most of the forgetting occurred very rapidly (in a matter of seconds), we would not detect it. Second, the retention intervals were always filled with memory tests on other tasks. If there is an output interference which cuts across tasks, sharp forgetting would have been found. Third, in general, the longer the retention interval the longer the subjects spent on the retention tests. We presented data on these intervals only for recall, but the same relationship was present for the other tasks in spite of the fact that for these other tasks (F, O, D) the subjects were required to respond to all items. The positive correlation between the length of the retention interval and the time the subjects spent on the retention tests could have influenced the amount of retention. It would do so if time spent on the retention tests and performance are related. We think it might have had a small effect on recall, but its influence on the other tasks is doubted. It is remotely possible that the influence of the three factors mentioned here could be in precise balance and result, therefore, in no evidence for forgetting when in fact there was forgetting, but we doubt it. Rather, we tend to believe that memories established under conditions of simultaneous learning may be particularly resistant to forgetting.

Finally, we were curious about the independence of the memories established for the four tasks. When we speak of independence we are referring to the independence of underlying processes. The correlational evidence could be interpreted to indicate a high degree of independence. This conclusion could also be supported by the failure of the memory test on one task to interfere with the retention of another task. Yet, in the long run, we must not only examine the correlations among tasks learned simultaneously, but also the correlations among these tasks when learned sequentially. Obviously, if the performances are uncorrelated when the tasks are learned sequentially, we could not conclude that differential encoding resulted from simultaneous learning.

MAJOR ANALYTICAL STEPS

The simultaneous presentation of four classes of items for learning in Condition FORD seems to have provided a rather severe challenge to the perceptual-memory system. As we proceed to the analytical stage, there will be an easing of the demands on the system, the easing being produced either by a reduction in the amount of information which the subjects were instructed to learn, by the amount of information they were presented, or by both. When these manipulations were carried to the extreme, the subjects were presented a single task to learn, and at this point we were in direct contact with the major streams of research. We will look at the major issues which prompted the analytical steps.

In carrying out the manipulations in which the demands on memory were reduced, the exposure period (study time) remained the same as for Condition FORD. It would seem beyond reasonable doubt that given a constant period of study, the greater the number of different tasks the subjects are required to learn, the less of each that would be learned. Nevertheless, the relationship between amount learned and study time is probably not a simple one when more than one task is involved. For example, in the simultaneous learning of two or more tasks, the subject may classify the items on a slide as belonging to different categories (based on orthography or on meaning) and this classification process may take disproportionately more time as the number of tasks increases. At the same time, it could be argued that the act of classification is an act of learning and therefore that classification time is not "lost" time. Thus, while it seems fairly sure that with a constant study time performance will improve as the number of tasks required to be learned decreases, the interpretation of the factors mediating the relationship may not be simple.

We have suggested the possibility that when the subjects' memory systems are overwhelmed, they might attempt to cope with the situation by attending to fewer than four tasks. We do not believe that this is likely to happen until the subjects have had an opportunity to get a clear idea of the magnitude of the task which faces them. In particular, we believe that a voluntary narrowing of effort to less than the number of tasks presented would occur after the first test trial when the subjects will have gained first-hand knowledge of what is expected of them on the tests. As we have mentioned earlier, the relatively high intertrial correlations of performance on three of the four tasks would be interpreted to mean that the subjects did not attend to fewer than four tasks. The argument is weak because we do not know what the intertrial correlations would be if no restriction of attention were possible (as would be the case when a single task is presented).

The evidence which will evolve from the conditions soon to be described will provide fairly definitive answers to questions concerning restriction of attention.

To repeat ourselves, we have said that when two or more tasks are given simultaneously, the subjects may classify each item as belonging to a particular category. If the subjects are instructed to learn only two of the four tasks, the classification may involve two levels, namely, those to be learned and those not to be learned, and, within those to be learned, the type of encoding to be used for each. If the subject does classify at the first level mentioned, and if the act of classification produces learning, it will be detected by a measure of incidental learning. A number of our conditions involve tests for incidental learning.

Because of the low correlations in the performances on the four tasks of Condition FORD, we suggested that the subjects may have encoded each task independently. Thus, in sequence, the subjects classified an item into one of four categories, and then encoded it to fit the demands of the tests, with the encoding of the items in the four categories being distinctly different from each other. This presumed orderliness is patently in contrast to other descriptions we have used, e.g., the overwhelming or the flooding of memory. Of course, the latter descriptions are not to be taken literally, and they represent more of the experimenter's view of the situation than of the subjects' view. The facts are fairly clear; the subjects did learn in Condition FORD; they did show improvement from the first to the second trial for three of the four tasks; they did handle the task given them without going into shock. The low intercorrelations among tasks were not symptoms of chaos. They will need interpretation and we anticipate the data evolving from the analytical steps will be useful for this purpose.

The Experimental Conditions

There were 20 different conditions, and three general variations among the 20 conditions may be identified. First, the number of tasks presented on the study trials was held constant at four with the number of the tasks to be learned being specified by instructions. Tasks not specified in these instructions were tested for incidental learning. The second general manipulation involved the number of tasks presented for learning, these paralleling the number of tasks to be learned as specified in the instructions for the first manipulation. In the third variation, two different tasks were always presented on the study trials, but the subject was instructed to learn only one of them, the other task being tested for incidental learning.

Symbol System

With 20 different conditions, there was a problem in deriving a code that would allow easy identification of each condition, although such a code seemed necessary. Here is what we did. Condition FORD, it will be remembered, was used to symbolize the simultaneous learning of four tasks: Task F, frequency assimilation of state names; Task O, the ordering of the seven street names; Task R, the recall of the company names, and Task D, the recognition of road signs. Using these four letters, each condition can be identified by two rules. First the presence of a letter indicates that that task was included on the slides. Second, if a hyphen is used to separate letters, those tasks represented by the letters to the left of the hyphen were (by instructions) tasks for intentional learning, those to the right of the hyphen were tested for incidental learning.

The 20 conditions are shown in Table 3. For expository purposes, they are shown in three columns. The meaning of some of the conditions will be indicated to show how the symbol system works. The first condition in the first column is F-ORD. In this condition (and all other conditions in the column), all four tasks were presented (just as in Condition FORD), but the subjects were instructed to learn only the frequency of the state names. But, they were also tested for the incidental learning of Tasks O, R, and D. In the last condition of the first column (FRD-O), the subjects were instructed to learn Tasks F, R, and D, but were also tested for the incidental learning of Task O. None of the conditions given in the second column involved incidental learning. In Condition F, only the state names occurred on the slides and the subjects were tested for frequency information. In Condition FRD, the slides included the item for Tasks F, R, and D, and the subjects were instructed to learn all three tasks and were tested for all three. The conditions in column three all involved one intentional task and one incidental task, with all possible combinations of the three tasks being included. As noted earlier, we chose not to include Task O in any of the manipulations shown in the second

Table 3

The 20 Conditions Used in the Major Analyses

F-ORD	F	F-R
R-FOD	R	F-D
D-FOR	D	R-F
FR-OD	FR	R-D
FD-OR	FD	D-F
RD-OF	RD	D-R
FRD-O	FRD	

Tasks: F = frequency judgments of state names

O = street ordering

R = recall of company names

D = recognition of signs

Tasks to the left of hyphen were intentionally learned, those to the right were tested for incidental learning.

and third columns.

Now, of course, the critical question is what we expected to learn from the results of these 20 conditions. However, it seems well to indicate first the procedures involved in these conditions.

Procedure and Subjects

The procedures for Condition FORD were given in detail, and the procedures for the conditions in Table 3 were exactly the same except when disallowed, or necessarily changed by the nature of the conditions.

1. The slides in all conditions were presented at a 20-second rate. Thus, even if only the items for one task were included on a slide (as in Conditions F, R, and D), each slide was presented for 20 seconds.

2. When less than four tasks were presented (as was true for all conditions in columns two and three of Table 3), the items held exactly the same positions on each slide as they had held for Condition FORD.

3. When two or more intentional tasks were included in a condition, the tests for the tasks occurred equally often as the first task tested, the second task tested, and, with three intentional tasks, as the third task tested.

4. There were always two study and test trials for intentional tasks.

5. Incidental learning was tested after the tests for the intentional tasks on the second test trial.

6. The instructions given initially always included a description of all of the classes of the materials (all tasks) represented

on the slide. If both intentional and incidental learning were involved, the instructions made it clear which task or tasks were to be tested. Thus, for Condition R-FOD, all four classes of material were described, and then the experimenter said: "We are primarily interested, in this experiment, in how well you can recall the names of the commercial companies. After you see the slides, you will be asked to write down as many of those company names as you can remember. Therefore, while you are studying the slides, you should concentrate on the company names."

After the second test trial on the intentionally-learned tasks were completed, the experimenter said: "Now I have three (two, one) more short memory tests for you to take. These tests are on the extra materials that were on the slides that you studied. We would like to see how much of this information you can remember even though you were not deliberately concentrating on it." By these means, incidental learning was measured.

7. As was true for Condition FORD, subjects were tested in groups of six subjects, with forms and conditions randomized across these small groups. There were 36 subjects placed in each condition. As described earlier, the seven conditions in the first column of Table 3 were included in a common testing schedule for 1974-75, and the remaining conditions in a common schedule for 1975-76. However, (as also noted earlier), because Condition FORD was also included in

these schedules, and because the results for the FORD conditions did not differ, we have concluded that all 20 conditions shown in Table 3 may be compared without a concern that the ability levels of the subjects across conditions may have differed appreciably.

<u>Data Analysis</u>

Some of the general theoretical issues which we believed would be touched upon by the data from the 20 conditions were indicated earlier. For the time being, we need to look at the conditions in a more empirical way. It would be absurd to try to analyze the results for the 20 conditions simultaneously; it is necessary to break up the conditions in meaningful ways. We will indicate what we conceive to be some of those meaningful ways and, therefore, indicate the nature of the analyses which will be given for each of the tasks.

1. A comparison of performance on each of the three tasks as a function of the number of other tasks also being learned will be given most clearly by the conditions in the second column. At the same time, we can determine the degree to which each task "subtracts" from the total learning potentional or total learning resources. For example, by comparing the frequency estimations made in Conditions FR and FD, we can determine the degree by which the recall and recognition tasks subtract from the resources needed for frequency assimilation.

2. Comparisons among the conditions in the first column will show how intentional learning differs as a function of the number of other intentional tasks. There is, of course, a confounding present in that as the number of intentional tasks increases, the amount of

selection required decreases (number of incidental tasks decreases). We will deal with this matter when we present the data.

3. The comparisons which can be made among the conditions in column three provide evidence on the learning of a single intentional task as a function of the type of uninstructed (incidental) task. And, of course, we will also obtain evidence on the amount of incidental learning which occurs as a function of the type of intentional task.

4. When attention is directed to the first three rows of the first two columns and corresponding pairs of conditions in column three, it can be seen that we will have evidence on the intentional learning of each of the three tasks as a function of the number of incidental tasks (0, 1 and 3), but since Task 0 has so little influence, it essentially becomes 0, 1 and 2 incidental tasks.

Results

Frequency Assimilation

Two measures have been used to reflect the relationships between true frequency and judged frequency of state names (Task F). One measure was the product-moment correlation between the true frequency of the 10 state names and estimated frequency, with a correlation being determined for each subject. For statistical purposes, the correlations have been transferred to $\underline{z}'$, but all reported values for the conditions will be the $\underline{r}$ for each condition as retransformed from the mean $\underline{z}'$. As a second measure, we will at least sample the data which show the absolute judgments as a function of presented frequency. The frequency assimilation which occurred under incidental conditions will also be presented for each measure.

Correlation measure: Intentional learning. The mean correlations for each of the two trials for each of the 10 conditions having intentional learning are shown in Table 4. The 10 conditions are placed to correspond to the listing in Table 3, and Condition FORD is included for comparison purposes. The results in Table 4 will be examined from several different approaches.

1. The first evident fact is that the relationship between true frequency and estimated relative frequency is substantial in all conditions. The lowest correlation in the table is .76 (Trial 1, Condition FR-OD). Thus, in all of these intentional learning conditions the subjects showed high sensitivity to frequency differences. As the number of intentional tasks increased, there appears to have been a small decrease in the magnitude of the correlations, but there are irregularities in this relationship. The evidence is given by the four conditions in the middle column of Table 4.

2. A second noteworthy fact is the small increase which occurs between Trial 1 and Trial 2. Indeed, in some cases there are small decrements. Performance increases reliably ($\underline{p}$ = .01) for only four conditions (F-ORD, FD-OR, FD, F-D).

3. There is an inverse relationship between the number of incidental tasks and performance. This is shown most clearly by the increase in correlations from Conditions F-ORD to F-R and F-D to F. The comparisons between FR-OD and FR, and between FD-OR and FD also

show a small negative role for the incidental tasks.

4. Task R (company names) is the major task which governs the performance on Task F. This is quite evident when Task R is an intentional task. There are several illustrations. Performance under Condition FR-OD is no better than under Condition FORD and is poorer than under Condition FD-OR. The differences in the mean correlations between Conditions FD and either FR or FRD are highly reliable (p = .01). To state the generalization, we may say that Task D (involving the traffic signs) as an *intentional* task had relatively little effect on frequency assimilation whereas Task R did. Nevertheless, even the relatively small effects of Task D are statistically reliable; e.g., F versus FD (p = .01). There are even some suggestions in the data that Task R as an incidental task disturbs frequency assimilation more than does Task D when it is an incidental task, (F-R vs F-D), but the difference is not reliable statistically. The fact that performance is better under Condition F than under any other condition indicates that the presence of any other material, whether to be learned or not, produces at least a small decrement.

5. Task O (represented by the seven street names) had no influence as an incidental task on frequency judgments. This can be seen by examining the data for the five conditions in the first column or by the direct comparison of Conditions FRD-O and FRD.

All conditions having two or more intentional conditions were examined for possible effects of length of the retention interval. As presented earlier, the performance on Task F under Condition FORD did

Table 4

Intentional Frequency Assimilation (Task F)

The values are mean correlations (decimals omitted) for each of the two trials.

Condition	Tr. 1	Tr. 2	Condition	Tr. 1	Tr. 2	Condition	Tr. 1	Tr. 2
F-ORD	83	89	F	94	95	F-R	89	88
FR-OD	76	77	FR	81	82	F-D	90	94
FD-OR	83	88	FD	89	92			
FRD-O	82	78	FRD	82	78			
FORD	81	79						

not change reliably across three retention intervals. Among the remaining six conditions, only Condition FD-OR produced a reliable change (p = .01) associated with the two retention intervals. When the frequency judgments constituted the first test, the correlation was .89, but fell to .81 when preceded by the recognition test. There was no interaction with trials. We have no accounting for the results of this isolated case.

Correlation measure: Incidental learning. There were five conditions in which the incidental acquisition of frequency information was measured following the second test trial on the material given for intentional learning. Correlations were calculated between true and estimated frequency. The five conditions and the correlations were: R-FOD, .50; RD-FO, .53, R-F, .62; D-FOR, .66; D-F, .71. All of the correlations are below any of those given in Table 4 intentional learning, but all nonetheless indicate a clear relationship between true and judged frequency. For the five conditions in the order given above, there were only 2, 4, 2, 0, and 0 subjects showing negative correlations. The lowest mean correlation differs reliably (p = .01) from the two highest, but the second lowest differs only from the highest. Looking at the conditions involved, it appears that the common factor influencing the relationship is the nature of the intentional task. Relatively low incidental acquisition occurs when Task R is an intentional task, and relatively high correlations occur when Task D is the intentional task. The result for Condition R-F is somewhat ambiguous with regard to the principle, but since a corresponding differential effect of Task R and Task D on Task F was found for intentional learning, the generality seems reasonably secure.

<u>Raw measure: Intentional learning</u>. As we have said earlier, there is no reason to believe that the correlation measure should reflect anything different from that shown by the raw measure. The correlations merely provide a simple way of deriving a slope measure for each subject. It will be remembered that in Condition FORD the mean judgments increased a small amount from Trial 1 to Trial 2 and the amount was independent of true frequency. A similar small increase was observed for several of the other conditions, but not for all, and there seemed to be no reason for this lack of consistency. The increase (when it occurred) could not be interpreted as an improvement in performance because the slope did not change. A change from Trial 1 to Trial 2 which would indicate an increase in the slope would be given by the trials X frequency interaction if, at the same time, high-frequency items showed an increase in judged frequency, or low-frequency items showed a decrease in judged frequency, or both. It will be remembered that of the conditions given in Table 4, four showed a reliable increase in the correlations between Trials 1 and 2, thus, presumably, reflecting a sharpening of the relationship between true and judged frequency. An examination of the raw judgments showed that the trial X frequency interaction was reliable ($p = .01$) for three conditions and these were three of the four conditions which showed an increase in the correlations between Trial 1 and Trial 2. The failure of a "match" for the two response measures occurred for

Condition FD, and no reason for this failure has been found.

For plotting purposes, we have combined the two trials and have asked about the influence of number of intentional tasks on the judgments. This allows all conditions to be represented, although it ignores the number of incidental tasks which accompanied the intentional tasks. The conditions combined were as follows:

1. Intentional Task: F-ORD, F, F-R, F-D
2. Intentional Tasks: FR-OD, FD-OR, FR, FD
3. Intentional Tasks: FRD-O, FRD
4. Intentional Tasks: FORD (using 126 Subjects)

Figure 9 shows that for all sets of conditions the usual overestimation of low-frequency items and the underestimation of high frequency items was present. When Task F was the only intentional task, the relationship between true and judged frequency was almost perfectly linear, whereas for the other conditions the sharp rise in the judgments between frequencies one and three disturbed the linearity. As would be anticipated from the relatively small differences among the correlations in Table 4, the differences in the slopes of the curves were not great. The number of intentional tasks does appear to have influenced the slopes, although the reversal implied between the two lower lines would not have been anticipated by the correlational evidence, nor does it make any sense. Still, the point we emphasize is the strong relationship between true frequency and judged frequency for all intentional learning conditions.

Raw measures: Incidental learning. The correlational data have clearly indicated that intentional assimilation of frequency information was far superior to incidental assimilation. We may now examine the raw judgments to obtain a more detailed picture of the nature of the differences. It will be remembered that incidental learning was measured only once for each condition, this measurement coming after two exposures of each slide. Therefore, the most appropriate comparison with intentional learning would be on the second test trial for the intentional tasks. The data for two intentional conditions have been chosen, those from Condition F and from Condition FORD. For the incidental learning, we have chosen to combine the two conditions producing the poorest performance (as judged by the correlational measure), and to combine the two conditions producing the best incidental performance. The former conditions are R-FOD and RD-FO, the latter, D-F and D-FOR. The results are shown in Figure 10. The performance under Condition F on the second trial was almost perfect in the sense that the mean judged performance at each frequency level was almost equivalent to the true frequency. There is only a slight overestimation at the low frequencies and only a slight underestimation at the high frequencies. The difference in the precision of the results for this condition and Condition FORD is quite obvious. Still, the performance under Condition FORD was far better than that under the conditions producing the best incidental learning. However viewed, it is evident

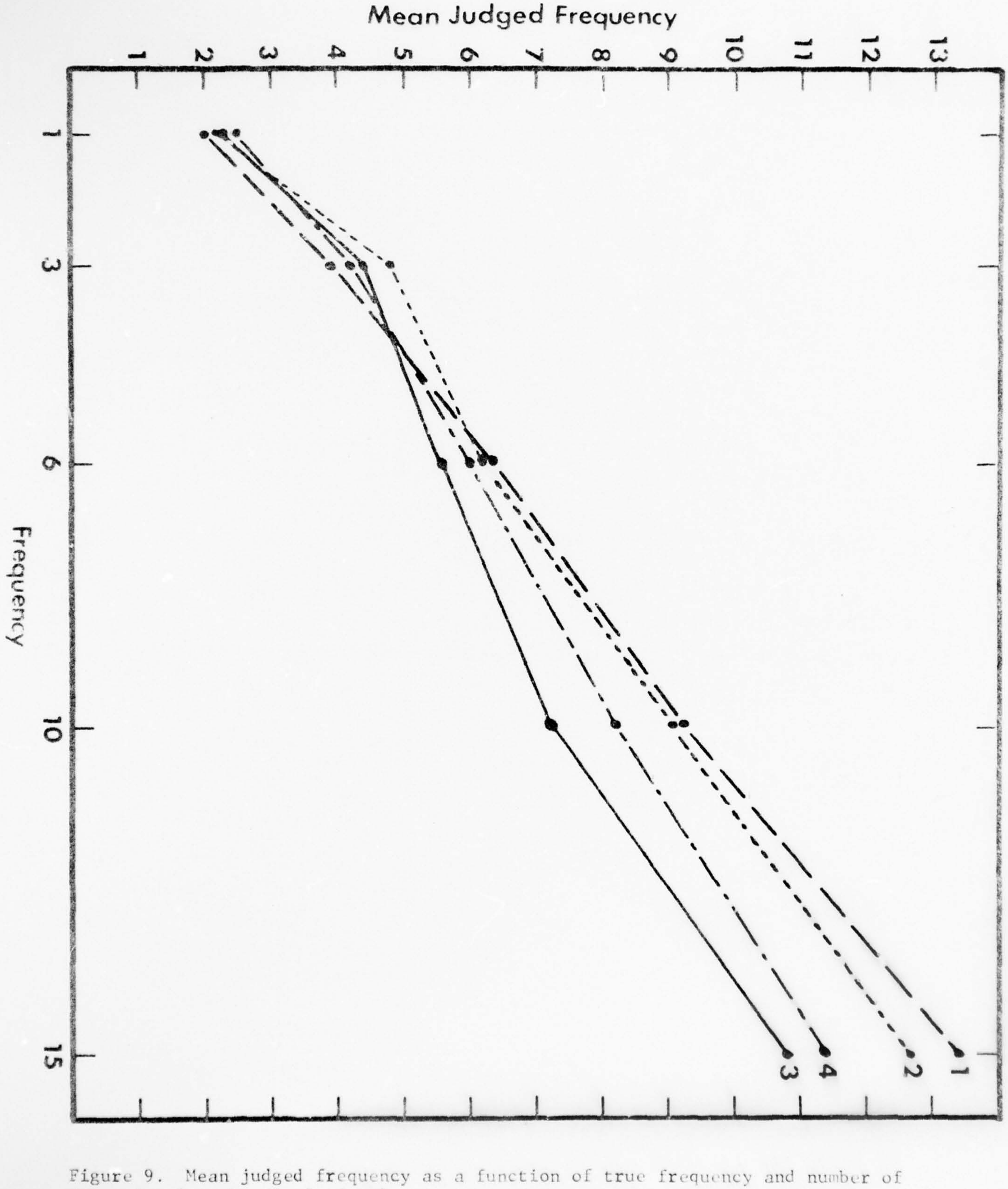

Figure 9. Mean judged frequency as a function of true frequency and number of intentional tasks (1, 2, 3, 4).

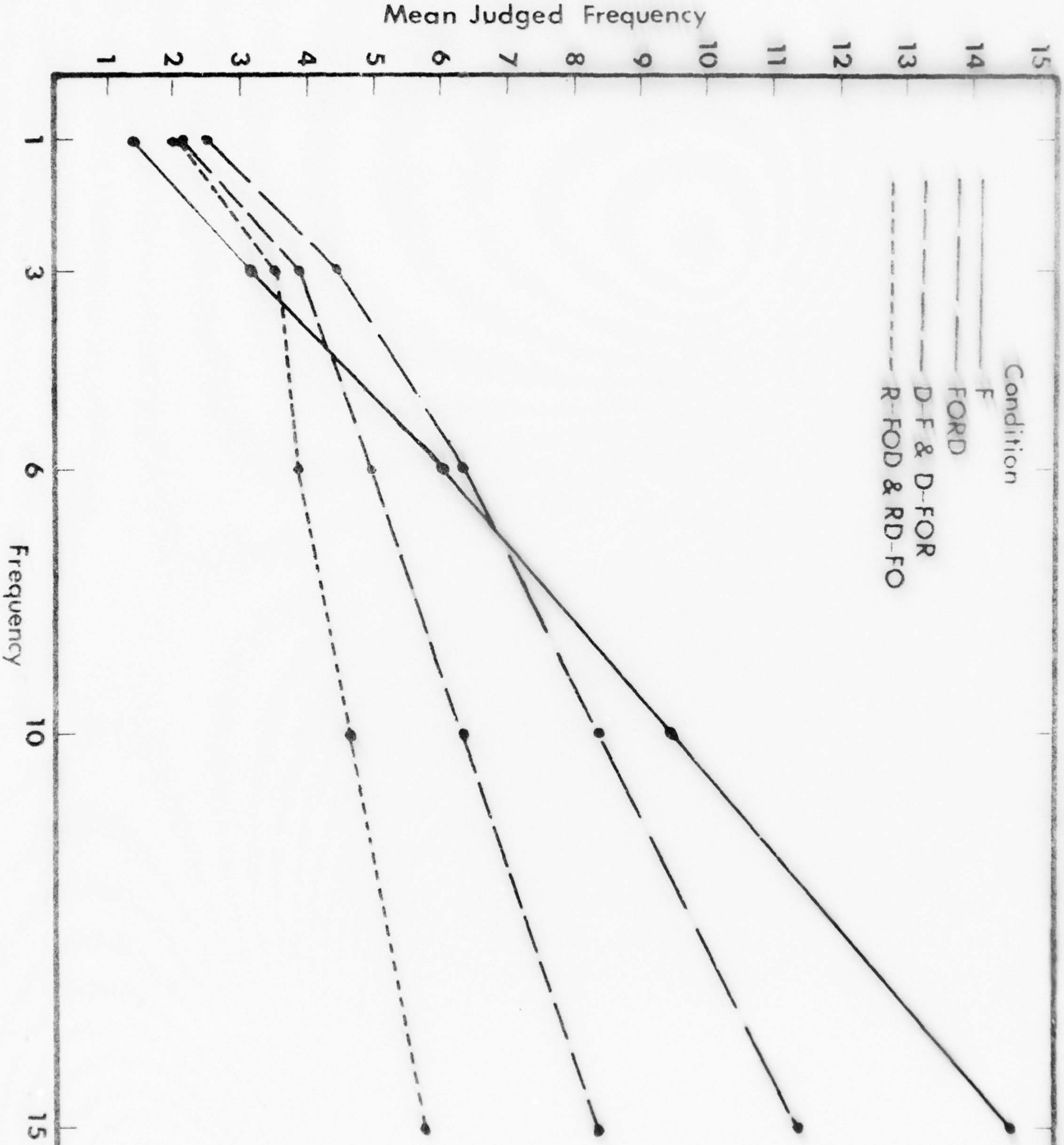

Figure 10. Mean frequency judgments as a function of true frequency and intentional and incidental learning. The two upper lines are the extreme cases of intentional learning, the two lower lines the extremes of incidental learning.

that under the conditions of the present study the acquisition of frequency information under all intentional conditions was far better than the acquisition under incidental conditions.

Summary. 1. The differences in frequency assimilation under all intentional learning conditions were relatively small. It is true that there were differences that were highly reliable statistically, but even in the conditions in which performance was poorest the correlative relationship approximated .80. Clearly, the number of intentional tasks influenced performance, as exemplified by the differences between conditions F and FORD in Figure 10, but the frequency knowledge which developed in Condition FORD was still very substantial. It is quite possible to argue that psychologically the difference in frequency information implied by correlations of .80 and .95 is far greater than implied by the numerical difference. In spite of these counter arguments, we will still maintain that the number of different intentional tasks was a relatively minor variable.

2. As a matter related to the first, we should indicate that the frequency judgments were in general degraded more when Task R was also an intentional task than when Task D was intentional. In fact, Task D had very little effect, and, as will be shown later, Task D was not heavily influenced by the intentional learning of other tasks. At the moment, there is no evidence to deny the possibility that when Task R was an intentional task it merely absorbed more of the study time than did Task D and therefore removed this time from being spent on studying frequency relationships.

3. In contradiction to other studies (Flexer & Bower, 1975; Rose & Rowe, 1976), frequency assimilation was poorer under incidental conditions than under intentional conditions. However, the differences among the methods of the various experiments are so marked that any serious attempt to rationalize the differences would be futile. It might be suggested that under the present incidental conditions the subjects may not have perceived some of the state names in a meaning sense. That is, it might be suggested that a subject could detect that a word on a slide was a state name without knowing which state name it was. Such a selection could be made by using the orthography as a distinguishing cue for words occurring alone. Thus, the single words presented as a part of Task D were all in upper case letters, with the state names in lower case except for the first letter. The question is, of course, whether this discrimination was made without also identifying the particular state name, and we do not know the answer to this question.

Ordering Task

It will be recalled that for this task the subjects were asked to order seven names of streets which had occurred on slides 2, 6, 10, 14, 17, 20 and 23. Only in Condition FORD did this task occur as an intentional task, and as reported earlier, the mean correlations between judged order and true order were .47 and .89 for Trials 1 and 2, respectively. There were seven conditions in which Task O was tested as an incidental task (left column, Table 3). The mean correlations between true and judged order varied between .05 and .39. The differences among the seven values were not reliable (F = 1.41), but the mean (.18) for the 252 subjects as a group was significantly greater than zero ($\underline{F}$ = 25.47). We will not dwell on these results, since this task has not entered into the intentional learning manipulations. As is obvious, a minimal amount of learning occurred incidentally, far below that shown under the intentional learning which occurred at the same time that three other tasks were being learned (Condition FORD).

Recall

The basic variables manipulated in the recall (Task R) of the 30 two-word company names will be reviewed. First, there was the spacing variable which had three levels (0, 1, and 3). All company names were presented twice. With 0 spacing, the two occurrences were on the same slide; with spacing 1, the two occurrences were on adjacent slides, and with spacing 3, the second occurrence was on the third slide following the slide of first occurrence. Second, there was the Same-Different variable, referring to the same or different spatial location of the two occurrences on the slides. Third, when there were two or more intentional tasks, there were retention intervals of different durations. There were two or more intentional tasks in seven different conditions. Fourth, there were two trials, and fifth, incidental recall was measured for five conditions.

As an initial step in the data reduction for the 10 conditions involving intentional learning, an analysis of variance was performed on each of the 10 conditions separately. Each condition showed a significant ($\underline{p}$ = .001) improvement in recall from Trial 1 to Trial 2; each condition showed a significant spacing effect ($\underline{p}$ = .001). None of the seven conditions having different retention intervals showed that the interval was a relevant variable. The Same-Different variable had inconsistent effects statistically, as will be shown at the appropriate time. None of the 10 conditions produced interactions between variables which reached the .01 significance level. The upshot of these initial analyses is that the retention interval and trial effects need not enter into further discussion. Thus, the major attention will be given to the spacing variable, the Same-Different variable, the number of intentional tasks, and the number of incidental tasks. Although all statistical analyses were performed on raw recall scores, the values used in tables and figures are percent recall of total possible.

Number of intentional and incidental tasks. We will first examine the influence of number of other intentional tasks on overall recall. The data are shown in Table 5, where the conditions are aligned in columns to correspond to the listing in Table 3, and again we have included the results for Condition FORD. As might be expected, maximum recall occurred under Condition R. There are some variations among conditions which cannot be accepted by any sensible considerations. There is no reason why Condition FRD-O should have produced slightly better recall than Condition RD-OF, nor is there any reason why Condition FRD-O should yield better recall (p = .01) than Condition FRD. We can only presume that sampling variations are involved and that among these several conditions recall is essentially equivalent.

Table 5

Percent Recall Averaged Across the Two Trials

Condition	Recall	Condition	Recall	Condition	Recall
R-FOD	36.4	R	47.2	R-F	43.4
FR-OD	29.3	FR	28.6	R-D	45.4
RD-OF	23.9	RD	26.4		
FRD-O	26.5	FRD	17.5		
FORD	19.2				

Even a casual examination of Table 5 would show that recall is inversely related to the number of intentional tasks. Using all of the data in Table 5, the 11 conditions may be combined into four groups to show the relationship between recall and 0, 1, 2, and 3 other intentional tasks, ignoring the number of incidental tasks. The groupings are: 0 (R-FOD, R, R-F, R-D); 1 (FR-OD, RD-OF, FR, FD); 2 (FRD, FRD-O) and 3 (FORD). The outcome is seen in Figure 11 which shows that performance decreases regularly as the number of intentional learning tasks increases, although the largest decrease occurred between 0 and 1 intentional tasks.

In contrast, several comparisons in Table 5 show that the number of incidental tasks had a minor influence on recall. Conditions R, R-F and R-D combined, and R-FORD in order represent 0, 1 and 3 incidental tasks, with corresponding recall values of 47.2, 44.0, and 35.4%, or a decrement of about 10%. On the other hand, Conditons FR and FR-OD did not result in a difference in recall, nor did RD and RD-OF. In both cases, the comparison represents 0 versus 2 incidental tasks.

For the results of the frequency judgments presented in an earlier section, Task R as an intentional task degraded the judgments more than did Task D. For the recall, there is no consistent evidence that the nature of the other intentional tasks (Task F or Task D) differed in their influence on recall. Conditions R-F and R-D did not produce reliable differences in recall, and neither did Conditions FR-OD and RD-OF.

The spacing effect. The percent recall (both trials combined) for each of the 11 conditions at each spacing level is shown in Table 6.

As noted earlier, for each condition the effect of spacing was highly reliable (p = .001). For all 11 conditions, performance increased sharply between spacing levels 0 and 1, and in 9 of the 11 conditions, performance continued to improve between spacing levels 1 and 3. As described earlier, none of the conditions showed an interaction between spacing and trials in spite of the fact that it might seem that the massed property of a spacing of zero would be lost over two trials. The exception occurred for Condition FORD (Figure 4) in which it was found that the interaction between trials and spacing was highly reliable, and it occurred with all three replications when tested separately. However, the interactions for Condition FORD indicated that the spacing variable was more potent on the second trial than on the first. We have not been able to find a reason for this discrepancy between Condition FORD and the other 10 conditions which is completely consistent across all conditions.

The magnitude of the spacing effect has been examined in conjunction with the other variables, and the striking fact is the constancy of its magnitude. For example, the number of other intentional tasks has little effect, the change in performance between spacing 0 and spacing 3 being from 10-12% when groups are combined in the manner used to construct Figure 11. An interaction with the Same-Different variable occurred only for Condition FORD.

Same-Different. The results for the spacing variable indicated a phenomenon of great probity. In contrast, the Same-Different manipulation produced results which, at least statistically, seem to be immune to rationalization. It will be remembered that a statistical analysis was

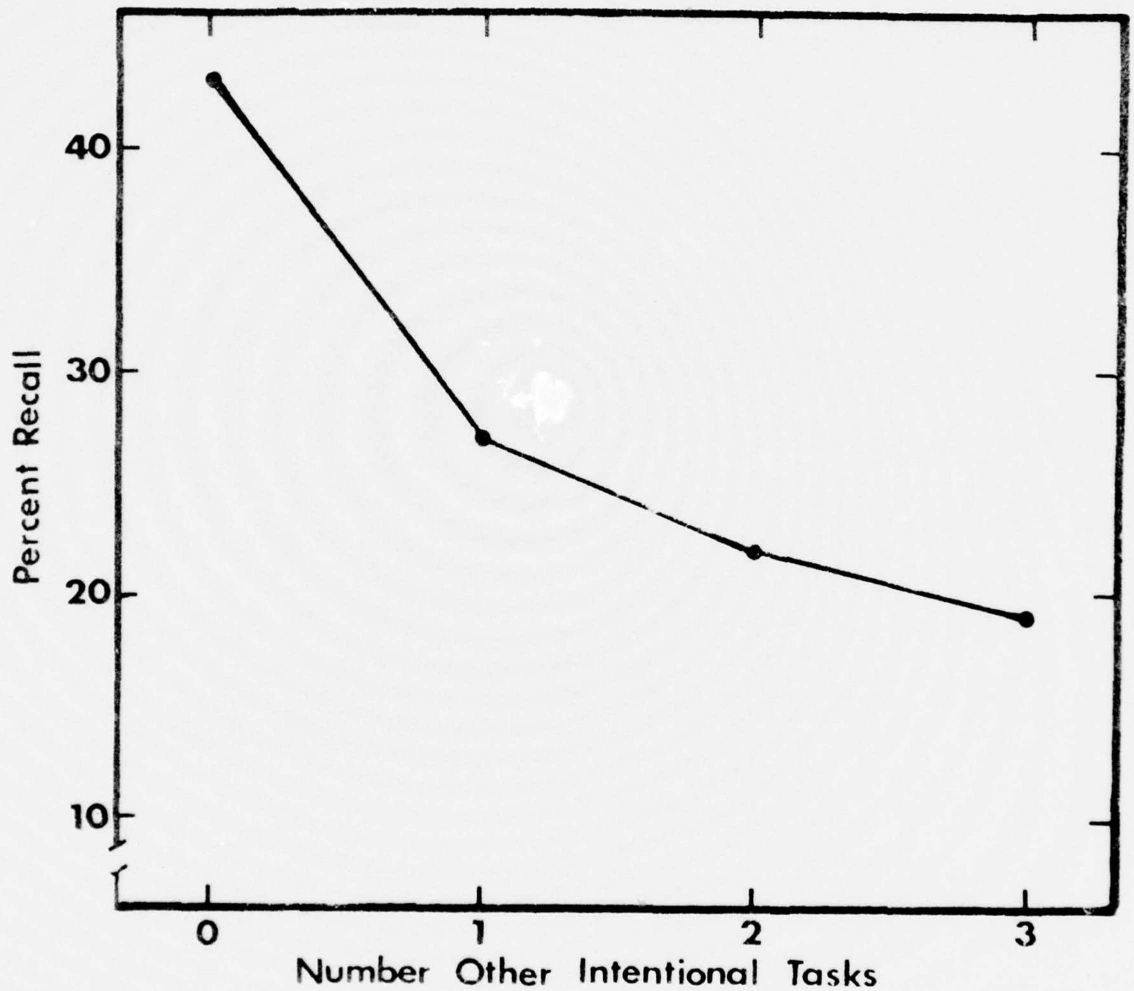

Figure 11. Percent recall as a function of the number of other intentional tasks.

Table 6

The Spacing Effect

(Each entry represents percent recall)

Condition	Spacing			Condition	Spacing			Condition	Spacing		
	0	1	3		0	1	3		0	1	3
R-FOD	30.0	36.8	42.5	R	42.1	46.5	53.1	R-F	35.7	42.6	51.8
FR-OD	23.3	32.4	32.1	FR	24.0	29.6	32.1	R-D	36.9	46.9	52.4
RD-OF	18.6	25.3	27.9	RD	19.7	27.5	31.9				
FRD-O	22.6	26.0	30.8	FRD	11.9	18.3	22.4				
FORD	14.6	21.8	21.2								

performed on each condition initially. Although 10 of the 11 conditions showed that recall was better when the company names had a constant (Same) spatial position on the slides on the two occurrences than when they occupied different spatial positions, the magnitude of the effect was extremely variable. At one extreme (Condition R-FOD) the difference yielded $\underline{p}$ = .001, while at the other extreme (Condition FORD), $\underline{p}$ = .26. Only Condition R-F showed that Different resulted in slightly better recall than did Same, but its companion condition (Condition R-D) showed the opposite effect ($\underline{p}$ = .003). Only Condition FORD showed an interaction with spacing (Figure 5). No condition produced a reliable ($\underline{p}$ = .01) interaction with trials. Across the 11 conditions the average of the two trials was 33.3%, for the Same condition, and 29.1% for the Different condition. The maximum difference observed for any condition was 7.0% (Condition R-D).

It would appear that the most conservative conclusion is that we are dealing with a variable that has a small effect on recall, and that the differences seen among conditions represent sampling variations around the true mean. Nevertheless, it should be noted that the conclusion that a constant spatial position gives better recall than a variable spatial position is consistent with other findings (e.g., Sohn, 1967).

Incidental learning. In spite of the fact that incidental recall was very low, the data proved to be related systematically to the independent variables. The percent recall for the five incidental conditions was : FD-OR, 3.6%; F-ORD, 5.6%; D-FOR, 6.0%; F-R, 8.1%; D-R, 8.3%. Application of statistical tests may be questioned because of a large number of zero entries. Nevertheless, for what it is worth, the $\underline{F}$ among the conditions was highly reliable (p = .01). The scores sort themselves into three levels (FD-OR, F-ORD and D-FOR, F-R and D-R). These three levels indicate that both number of intentional tasks and number of incidental tasks influenced incidental recall. Recall is poorer with three incidental tasks than with one, and with two intentional tasks, performance is poorer than with one.

Without exception, for each condition, performance increased as spacing increased, and performance was better under the Same condition than under the Different condition. We have combined the five conditions in Figure 12 as a means of demonstrating the orderly character of the data. The fact that the spacing variable is effective even under incidental conditions has been demonstrated by other investigators (e.g., Shaughnessy, 1976). So far as we know, no previous study has evaluated the effect of the Same-Different variable under incidental conditions.

Miscellaneous. In a previous section, recall was examined as a function of the position of the items in the study list and as a function of the retention interval for Condition FORD (Figure 6). Essentially the same results were found for Condition FRD-O and FRD. That is, although the retention interval was not a significant source of variation, position was. Recency effects were noted on the first recall trial, with both primacy and recency effects on the second test trial. A similar effect was found with conditions having two intentional tasks (hence two retention intervals). With only a single intentional task, the position effects followed the same pattern but were much more marked.

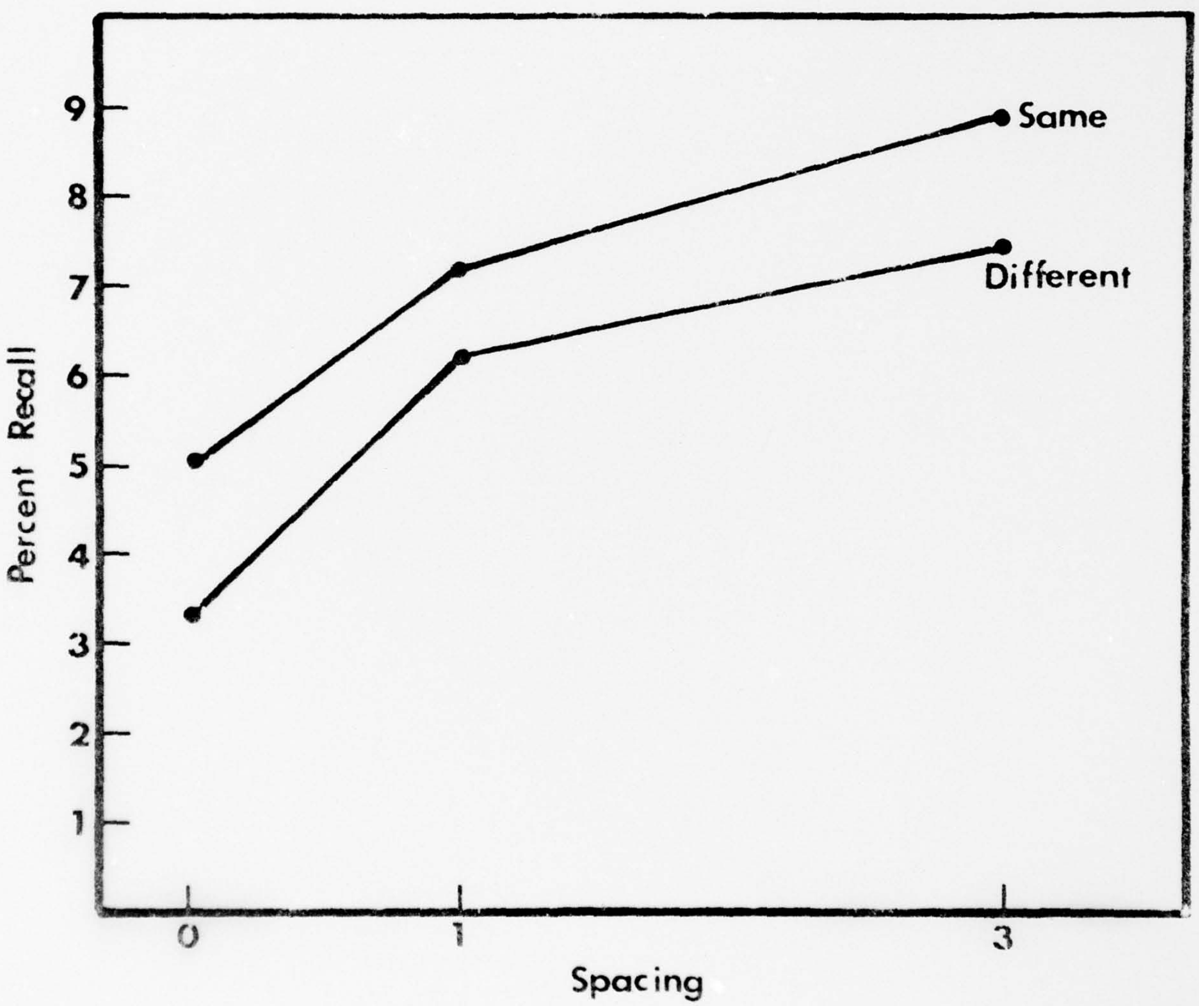

Figure 12. Percent incidental recall as a function of spacing and the Same-Different variable.

A major concern has been with recall when from one to three other tasks were being acquired simultaneously. It might be expected that item difficulty could be changed appreciably as the amount of other material on the slide varied. Thus, any item that is relatively easy when presented alone for learning might possibly become relatively difficult when occurring on a slide in which there were state names, traffic signs, and street names. This seems not to have been the case. We determined the number of times each company name was recalled when summed across the three forms (in order to minimize position effects). This was done for Condition FORD and for Conditions R, R-F, and R-D combined. Condition FORD represents a "cluttered" series of slides, the other three conditions a relatively uncluttered series of slides. The correlation between the two distributions of 30 scores was .90. Clearly, an item's difficulty seems to be based largely on something quite apart from the context in which it is presented.

Summary. 1. We have found, not unexpectedly, that recall of two-word company names decreased directly as the number of other tasks being learned increased. The variation extended from 47% when only items for recall were presented, to 19% when three other tasks were being learned simultaneously.

2. The effects of number and type of incidentally presented tasks on intentional recall were small.

3. Spacing effects were found under all conditions, including incidental learning. These effects appear to be universal.

4. The Same-Different variable had a small effect overall but the magnitude varied considerably and unsystematically from condition to condition.

Recognition

There were three classes of items included on the recognition test (Task D), namely, 12 traffic signs which occurred once on each of the study trials; 12 signs for which the two words had appeared on different slides on the study trials, and 8 signs which appeared as new signs on the test trials. The subject made YES-NO decisions on the test as to whether the two-word signs had or had not appeared as intact signs on the study trials. When the two words constituting a sign were separated on the study trials, the second word occurred on the 1st, 2nd or 4th slide following the one on which the first word occurred.

The recognition results will be organized around two measures. First, as a measure of sensitivity or discriminability, the sum of the misses on old items and false alarms on new items was used. The second measure simply consisted of the false alarms on the separated items, but to assess meaningfully the frequency of these false alarms, the frequency of false alarms on the new signs was used as a control.

When two or more intentional tasks were given the subject, retention intervals varied in length. But, as has been true for the other tasks, Task D showed no systematic changes as a function of the length of the retention interval and so this variable will not enter into the results which follow.

Misses plus false alarms. The performance on each trial for the 11 conditions is shown in Table 7. Among the 11 conditions, fewest errors occurred for Condition D (as might be expected). It is, perhaps, somewhat

Table 7

Recognition as Measured by Misses Plus False Alarms

(The entries are percent errors)

Condition	Trial		Mean	Condition	Trial		Mean	Condition	Trial		Mean
	1	2			1	2			1	2	
D-FOR	17.9	3.8	10.9	D	12.8	4.9	8.9	D-F	14.7	7.4	11.1
FD-OR	22.6	11.7	17.1	FD	14.8	7.2	11.0	D-R	18.2	7.9	13.0
RD-FO	22.3	13.7	18.0	RD	21.3	11.5	16.4				
FRD-O	23.1	17.5	20.3	FRD	19.7	12.2	15.9				
FORD	24.2	14.4	19.3								

surprising that there were any errors at all on this condition on the second trial. The subjects were allowed 20 seconds for each slide, and across the 24 slides there were 12 intact signs which occurred once on each trial. Still, on the second trial there were 8.3% misses on these 12 items (about one item), although the false alarms were near zero. We suspect that this rather poor performance was not due entirely to the attempts by the subjects to be sure and remember the separated signs. We suspect that the long exposure periods for the single task is very ineffective programming if we ask about the amount learned per unit of time. This matter will receive more attention in later experiments.

The data of Table 7 show clearly that the performance decreased as the number of intentional tasks increased (ignoring the number of incidental tasks). The increase in errors was not large, but it was fairly consistent. When only Test D was an intentional task (Conditions D-FDR, D, D-F, D-R), there were 11.0% errors across the two trials. With one additional intentional task beyond Task D (Conditions FD-OR, RD-FO, FD, RD), the value was 15.6%. When two additional tasks were being learned (Conditions FRD-O and FRD) the errors averaged 18.1%, which is only slightly less than for Condition FORD. The increase in errors as a function of the number of other intentional tasks occurred for both the misses and for the false alarms. Thus, the discriminability between old and new items decreased as the number of intentional tasks increased, and this must be due to a lower level of learning for the old items as the number of intentional tasks increased.

Although we will not present data, we can report that we were unable to find differences in the criteria for responding among the various conditions.

The evidence indicated that when Task R (recall) occurred as an intentional task with Task F (frequency judgments), performance on Task F was poorer than if Task D was an additional intentional task. The present data are not consistent on this matter. That is, they do not tell us clearly whether Task R as an intentional task along with Task D produced poorer performance on Task D than did Task F as an additional task. A comparison between Conditions FD and RD indicates that Task R had a more potent negative effect than did Task F ($\underline{p}$ = .01), but this is not supported by the comparison between FD-OR and RD-FO ($\underline{p}$ = .67). It might be suggested that the lack of a difference for this latter comparison is due to the fact that Task R occurred as an incidental task in Condition FD-OR and produced a more severe decrease as an incidental task than did Task F. Such a conclusion is denied by other data (Condition D-F versus D-R). In fact, however, the role of the *incidental tasks* was not completely clear. Generally speaking, the presence of incidental tasks did increase recognition errors, but the reliability of the increase is evident in only one comparison (FD versus FD-OR). The difference between Conditions D and D-FOR is unreliable, but there is a trial by condition interaction ($\underline{p}$ = .002) which (as may be seen in Table 7) indicates a stable difference on the first trial. The appropriate conclusion, viewing the data overall, is that the incidental tasks have at best only a small effect on recognition performance.

Incidental recognition. Incidental recognition measures were available for five conditions. The percent errors (misses plus false alarms on new items) on these conditions were as follows: F-D, 18.4%; R-D, 22.2%; R-FOD, 23.0%; F-ORD, 23.0%; FR-OD, 27.4%. The differences among the five conditions was of borderline statistical reliability ($\underline{p}$ = .05), and, as has been our policy, we have chosen not to concern ourselves with such differences.

An examination of Table 7 will show that the poorest performance after two intentional study trials occurred for Condition FRD-O (17.5%), a value which is just slightly below that shown for the condition (F-D) resulting in the best incidental performance (18.4%). This means, of course, that for all conditions the intentional performance was superior to the incidental. The mean percent error on the second trial for the misses for all 11 intentional groups was 15.3%, and for the false alarms, 5.1%. The corresponding values for the five incidental groups were 33.6% and 12.0%. So, although the incidental learning demonstrated in the recognition data was far better than expected by chance, the amount of learning did not approach the amount of intentional learning.

Separation variables. The data for Condition FORD, as detailed earlier, indicated that the amount of separation of two words in the study list did not influence the likelihood that the subject would say that the two words had occurred together on a slide. Yet, the frequency with which such false alarms occurred was greater than the frequency of false alarms made to new pairs. We now will evaluate the effects of this variable for the 10 conditions.

As a first step we have combined conditions so as to ask about the false alarms on these broken pairs as a function of the number of other intentional learning tasks (0, 1, 2, and 3), the combinations being the same as described earlier several times. As may be seen in Figure 13, the degree of separation had no consistent effect on the number of false alarms.

Each of the 10 conditions was evaluated separately, and in only one condition (FD-OR) did the separation variable produce differences that approached statistical reliability ($\underline{p}$ = .03), but even here the differences were not systematic, the mean for the two trials being 22.2, 15.3, and 19.8% for separations of 1, 2, and 4, respectively.

Figure 13 also indicates that the frequency of false alarms was not influenced by the number of other intentional tasks given beyond one. Furthermore, the number of false alarms was totally uninfluenced by the number of incidental tasks present. For example, the mean percent false alarms across the two trials were 10.1, 13.5, 9.0, and 11.5% for Conditions D, D-F, D-R, and D-FOR, respectively.

We next ask about the frequency of false alarms to the broken items as compared to the control (new) items. In every conditions the numbers of false alarms were greater for the broken items than for the control. Only one condition showed a statistical deviation from the other 10 conditions. For this condition (D-R), the differences between the control and broken items, considering both trials combined, was only 1.5%. On the second trial there were 4.9% errors for the control, 4.6% for the broken items. It seems that we are destined to find a deviate condition in every task; we have been unable to find any reason for the essential absence of the effect for this condition. Except for this one condition, the small differences among the other conditions did not appear to result from any of the other variables, i.e., number of intentional tasks, or number of incidental tasks. That these differences were relatively constant may be in part at least understood by the fact that there were substantial correlations across subjects in their tendency to make false alarms of both kinds. Across condi-

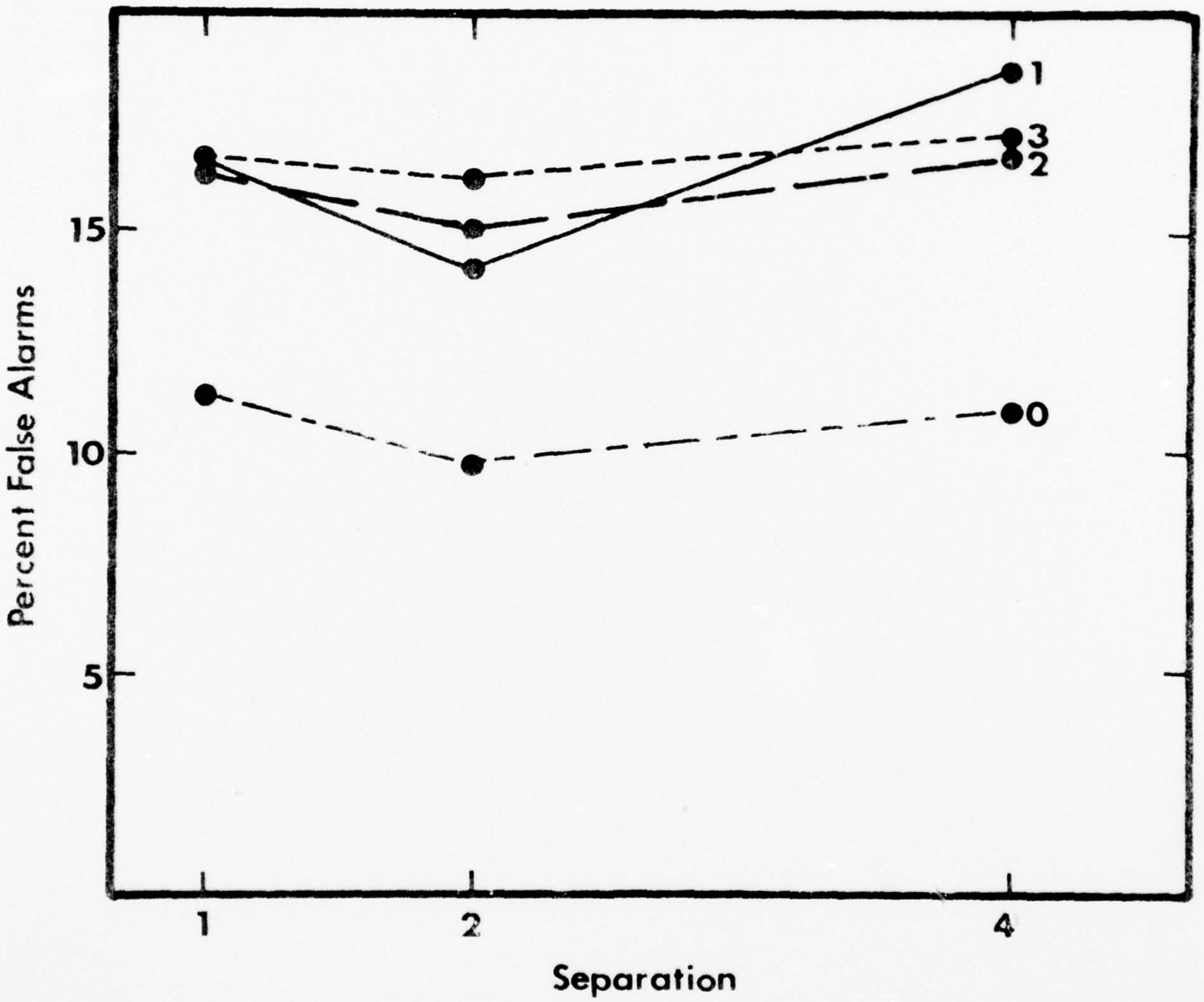

Figure 13. False alarms on broken items as a function of separation and number of other intentional tasks (0, 1, 2, 3).

tions these correlations ranged between .27 and .76, with a mean of .52. Thus, in general, as the number of false alarms on new items differed among subjects, so also did the number of false alarms differ correspondingly on broken items. The rank-order correlation between the two types of false alarms across the 11 conditions was .76. In fact, the only difference we have found between the false alarms for the two types of items was the small influence of number of intentional and incidental tasks on the broken items.

Because conditions had relatively little effect on the magnitude of the difference between the two types of false alarms, we have, by way of summary, combined the 486 subjects in the 11 conditions to construct Figure 14. The differences between the false alarms on the broken and control items on the first trial is approximately 10%, and reduces to 5% on the second trial. However, this obvious interaction is probably due in part at least to a "basement" effect for the control items on the second trial. It is possible that second-trial performance was influenced by the presence of the two-word signs on the first test trial. But, for both types of items, such a presence would be expected to increase the number of false alarms. We have not sought to pursue this issue analytically, but data to be presented later have some relevance to the matter.

Incidental learning. It will be useful at this point to present the results for the five conditions for the three classes of items which occurred on the incidental recognition test. The sums of the misses and false alarms (on control) was used earlier as a measure of sensitivity. It can be seen in Table 8 that the number of false alarms on the broken pairs varied between 28.3% and 41.7%, with a mean of 35.4%. Statistically, the differences were not reliable (F = 1.70). When the mean (35.4%) is compared with the mean percent false alarms on the control items (12.0%), it is evident that the presence of the single words on the slide during study was a very strong lure when the subjects had to decide whether or not they would accept the two-word signs as having been presented as two-word signs during study.

The five groups did differ reliable (p = .01) on the number of false alarms made to the control items. An inspection of Table 8 will show that the five conditions sort themselves into two groupings. When the intentional task was frequency assimilation (F-ORD, F-D), the number of false alarms was relatively low. When the intentional class included recall (R-D, R-FOD, FR-OD), the number of false alarms was relatively high.

A survey of the data for the misses shows that they were not influenced by the nature of the intentional task. Indeed, the differences among the five conditions were not reliable (F = 1.83), but even the small differences which exist do not correspond to differences that could be produced by the type of intentional task. Thus, the data indicate that the number of false alarms to control items was governed in part by the nature of the intentional task, but that this was not true either for the misses or for the false alarms on the broken items.

Data presented earlier for intentional recognition indicated that the numbers of false alarms to control item and to broken items were correlated within each group of subjects. The five comparable correlations for

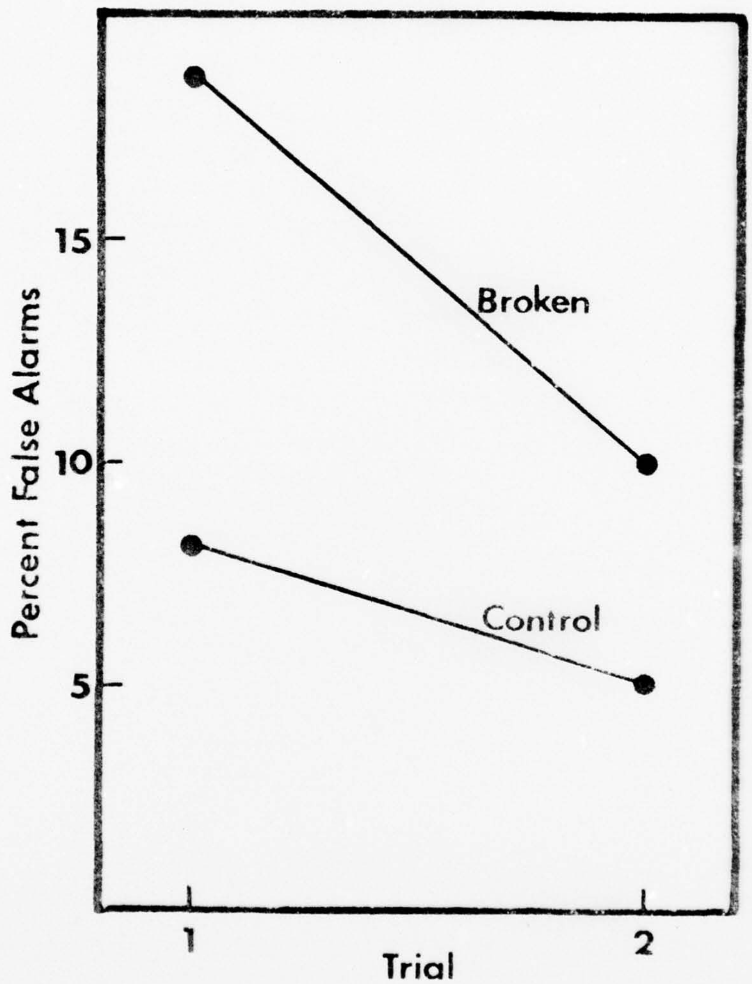

Figure 14. False alarms on broken and control items as a function of trials.

Table 8

Percent Recognition Errors on Various Classes of Items

Under Incidental Learning

Condition	Misses	False Alarms Control	False Alarms Broken
F-D	30.6	6.3	28.3
R-D	32.2	12.2	41.7
F-ORD	37.3	8.7	35.9
R-FOD	29.4	16.7	36.3
FR-OD	38.4	16.3	35.2

the incidental groups were all positive but low, varying between .13 and .39, with a mean of .24 (versus a mean of .52 for the intentional groups).

Summary. 1. The analysis of the last of the four tasks has once again shown that performance was not influenced by the length of the retention interval.

2. Recognition performance, as measured by the sum of the errors on old and new items, decreased as the number of intentional tasks increased. The data did not give a clear decision concerning which of the other two intentional tasks influenced recognition more severely. But it was found that the false alarms on new items measured under incidental learning were more frequent when Task R was the intentional task than when Task F was the intentional task.

3. The number of incidental tasks had at best only a small effect on intentional recognition.

4. Some two-word signs were presented so that the first word occurred on one slide, the second on a later slide. When the subjects were asked to identify the two-word signs which had been presented as two words on a slide, the number of false alarms was more frequent than for control items, and the magnitude of the difference was relatively constant across all intentional conditions. The degree to which the two words were separated on the study slide was quite irrelevant. The number of the false alarms on these broken items was increased by the presence of a second intentional task, but not by any further tasks, and the number of incidental tasks had no influence.

5. Although incidental recognition scores were poorer than intentional scores, there was nevertheless information appropriate for the recognition test which was acquired incidentally. It was particularly apparent

that the subjects knew a great deal about what had not been presented as indicated by the small number of false alarms on new items.

Correlational Evidence

Reliability. The reliability of the tasks as measured by the correlations between trials one and two are shown in Table 9. Condition FORD is also included for completeness. There are two conclusions we wish to draw from Table 9. First, there was some stability in the scores for each task from trial to trial, although the recall scores seemed to be somewhat more stable than were those for the other two tasks. Second, the reliabilities for recognition and frequency were not influenced systematically by the number of other intentional tasks which were involved, but recall was. The average correlations for 0, 1, 2, and 3 other intentional tasks when recall was involved were .86, .73, .66, and .64, respectively.

The fact that the intertrial correlations for recall decreased as the number of inteational tasks increased may suggest that at least some subjects spent a disproportionate amount of time on learning the company names on the second study trial. The reasoning would go as follows. As we have seen, when several intentional tasks were required, recall in an absolute sense was very low on the first trial. There seems to be little doubt that subjects could more readily recognize their poor performance on the recall tast than on the other tests. They might decide, therefore, to spend more time on the second trial than they did on the first trial in trying to learn the company names. If some subjects used this approach, the intertrial correlations would be reduced because these subjects would show a

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THE SIMULTANEOUS ACQUISITION OF MULTIPLE MEMORIES.(U)

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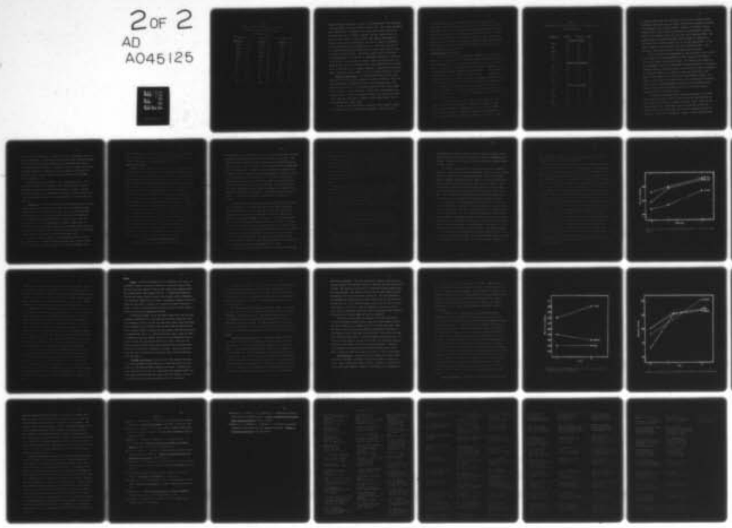

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Table 9

Trial 1 x Trial 2 Correlations as Measures of

Reliability for Intentional Learning

(decimals omitted)

Frequency (F)		Recall (R)		Recognition (D)	
Condition	r	Condition	r	Condition	r
FRD-O	67	FRD-O	75	FRD-O	79
FD-OR	53	RD-FO	80	RD-FO	60
FR-OD	37	FR-OD	64	FD-OR	63
F-ORD	63	R-FOD	74	D-FOR	63
FRD	72	FRD	57	FRD	37
FR	51	RD	67	RD	50
FD	57	FR	82	FD	37
F-R	57	R-D	90	D-R	54
F-D	63	R-F	83	D-F	50
F	45	R	85	D	68
FORD	62	FORD	64	FORD	56

bigger improvement from trial 1 to trial 2 than would subjects who maintained a constant amount of study on each task on each trial. But if this did happen, there should also be a reduction in the correlations for the other tasks -- for recognition and for frequency judgments, and such reductions were not observed as the number of tasks increased. What appears most probable is that the reduction in the correlations for recall as number of intentional tasks increased was due to a statistical matter. Recall was very poor on the first trial when three and four tasks were being learned, and the range of scores was sharply limited. Correlations simply cannot be high under these circumstances. It seems that the appropriate conclusion is that there is essentially no evidence that the subjects shifted attention so that the amount of time spent on the different tasks varied appreciably from the first to the second trial. Or, in a positive fashion, it appears that our subjects spent a relatively constant amount of time on each task from trial to trial.

Intertask correlations. The intertask correlations for each trial separately, and for both trials combined, are given in Table 10. Except for Condition FORD (the correlations for which we are repeating here), each correlation is based on $N = 36$, for which values of .33 and .42 are needed for $p = .05$ and $p = .01$, respectively. Because Task D (recognition) used an error measure, a positive relationship between its scores and the scores for the other two tasks would be shown by negative correlations. We have simply ommitted the negative signs.

The data in Table 10 show that performance on the frequency-judging tests and on the recall tests were quite unrelated. There was a weak relationship between frequency and recognition, and a somewhat stronger relationship (although still weak in an absolute sense) between recall and recognition. Much earlier the results for Condition FORD indicated that the processes underlying the three tasks were relatively independent. The data for the other conditions in which fewer than four tasks were learned simultaneously confirm the conclusion reached for Condition FORD. More generally, the number of tasks which were learned simultaneously did not influence the intercorrelations among the tasks.

General Summary and Discussion

It was not surprising to discover that performance on a given task decreased as the number of other tasks being learned simultaneously increased. But it was surprising to discover that the magnitude of this effect was so small. The influence of the additional tasks on frequency judgments was minor and quite irregular, although both recall and recognition performance decreased steadily as the number of other tasks being learned increased. In the case of the recall task, the difference in performance when no other task was learned and when three other tasks were learned was approximately 24% (see Figure 11). However, about 16 of these percentage points was caused by the adding of only one task; adding of additional tasks had relatively little influence. In effect, these additional tasks were learned with very little cost.

The data also showed that performance on a task decreased as the number of incidental tasks increased. Roughly, this decrease was about half the decrease produced by adding tasks to be intentionally learned. For all tasks, incidental learning was much poorer than intentional learning, but still there was some incidental learning for all tasks. Indeed,

Table 10

Intertask Correlations for Trials 1, 2, and Both Trials

(Decimals omitted)

Condition	Trial 1	Trial 2	Both
	Frequency x Recall		
FRD-O	-03	10	02
FRD	12	16	14
FR-OD	21	15	17
FR	-14	03	-04
FORD	11	10	12
	Frequency x Recognition		
FRD-O	13	48	30
FRD	08	39	39
FD-OR	-03	42	17
FD	19	18	20
FORD	24	14	30
	Recall x Recognition		
FRD-O	27	45	40
FRD	11	33	27
RD-FO	48	42	41
RD	41	41	48
FORD	35	23	35

it might be argued that from a practical point of view the amount learned incidentally was greater than the amount of intentional learning lost because of the presence of the incidental tasks. Further, the argument could be advanced that a better strategy for maximizing learning within a constant period of time would be to give all four tasks (as in Condition FORD). The argument would be based on the premise that the amount of learning of a given task not realized when other tasks are learned simultaneously is less than the amount of learning which occurs on the additional tasks. Just how much is added would depend on the weighting given each task.

One of the reasons for undertaking the present studies was to discover if the effects of certain independent variables changed as the number of tasks being learned simultaneously changed. The answer to this inquiry is quite unambiguous for the few independent variables manipulated. The spacing variable produced an amazingly constant effect across the various conditions. The frequency judgments as a function of frequency varied only a small amount as the number of tasks being learned simultaneously changed. The number of false alarms on the broken pairs in the recognition task remained quite constant (when gauged against the control items) regardless of the number of other tasks being learned, and in no case did the degree of separation produce a systematic effect.

We presented intercorrelations among the performances on the three major tasks and interpreted them as being so low that there could not be much overlap in the processes underlying the three tasks. There are two implications of this finding which need to be mentioned at this time. First, the low correlations might lead us to the conclusion that the independence is due to the simultaneous learning. We might speculate that the subject, in order to handle the rather difficult situation he faces when learning two or more tasks, encodes each task differently. Because of the differences in orthography for the materials in the different tasks, the subjects can rather quickly categorize the items and then apply the code appropriate to the category. If distinctly different types of information lie behind the different codes, the low relationships among the tasks as seen in Table 10 would be anticipated. There is a problem with this approach. The correlations in Table 10 do not seem to differ in any systematic way as a function of the number of tasks being learned. It would seem that it would be easier to use two distinguishing codes (when only two tasks are learned) than four (when four tasks are learned), but no such distinction is reflected in the correlations. Of course, the critical data would be the correlations among the tasks when they are learned separately. We will turn to such data in due time.

The second implication of the correlations derives from a theory of recognition memory. This theory (Underwood, 1971) assumes that a major type of information used in recognition decisions is frequency information. The theory clearly leads to the expectation that the smaller the frequency differences between two events which can be discriminated, the better the recognition performance: those who can discriminate frequency differences well will do well on recognition tasks. The intercorrelations between frequency judgments and recognition performance in Table 10 are too low to give support to the theory. The condition of direct relevance is Condition FD, and for this condition the intercorrelations are not significantly different from zero. It still must be determined whether the failure of the theory is due to simultaneous learning (in which different codes are produced for the two tasks), or whether the theory is inappropriate for these tasks. The expected correlation has been found in other situations (Underwood, Boruch, & Malmi, 1978).

TIME FACTORS

Exposure Time and Simultaneous Tasks

We will report two studies in which the central variable was study time. The first study to be described was in fact the first conducted (other than the pilot study) as we set about to discover the phenomena which emerge as a subject is asked to learn several tasks simultaneously.

There were actually three conditions making up the first experiment in which a 10-second exposure of each slide was used. One of these conditions was Condition FORD. Immediately after completing these three conditions, we tested another group on Condition FORD using the 20-second rate. (The 54 subjects given the 20-second rate were included in the 126 subjects that produced the data for Condition FORD as reported earlier). It is possible, therefore, to compare the two groups for Condition FORD, one group having the slides presented at a 10-second rate, the other at a 20-second rate, hence we will report the results for four conditions.

Conditions

For all four conditions, the slides were the full slides as used in Condition FORD. Each of the four groups contained 54 subjects, and all details of forms, interval balancing, and so on, were exactly the same as has been described earlier for the other experiments. Two of the conditions will be designated as FORD-10 and FORD-20 to indicate the rate difference. For the two other conditions the rate was 10 seconds. They differed from each other, and from the two FORD conditions, as follows:

Condition FORD-NI. This condition was exactly the same as FORD-10, except the subjects were not instructed (NI) concerning the four classes of materials, nor how they were to be tested. They were told that they would see single words and two-word units, that some of the words would occur more than once, and that the words they saw would be like the words that they might, in fact, see in driving through an urban area. They were further told: "Of course, I am going to test you for your memory for the events you saw during the ride, but I am not going to tell you just how I will test you."

Condition FORD-NIT. The 54 subjects in this group were given the same initial instructions as were the subjects in Condition FORD-NI. That is, they knew they were to remember the words on the slide, but they were not informed about the classes of words nor how they would be tested. The difference between the two groups was that the subjects in Condition FORD-NIT were not given the usual first test trial (NT, not tested). After the first study trial, these subjects were given a 10-minute "rest", during which they tried to solve a series of anagrams. This 10-minute period approximated the time required to administer the first test trial to the other groups. After the 10 minutes had elapsed, the subjects were told that the slides would be shown for a second time in exactly the same order as on the first study trial, and that they would be tested after the second study trial (which they were).

For these four conditions, we will be making three different comparisons. First, a comparison of FORD-10 and FORD-20 will show the effect of two different exposure durations. Second, a comparison of FORD-10 and FORD-NI will show the effect of initial instructions concerning classes of items and the nature of the tests, the subjects in FORD-10 being fully instructed, those in FORD-NI not being fully instructed. The comparison will be most critical on the first test trial. Third, comparison between Condition FORD-NI and FORD-NIT will show the effect of the first test trial on the performance on the second test trial.

Results

In describing the results we have tried to avoid excessive detail, particularly detail which is in effect repetitious of data which have been presented in conjunction with other conditions. Our efforts will be directed primarily to unusual findings. When we speak of differences that were found we will be referring to differences which had probability levels of .01 or lower.

Exposure duration. The subjects in Condition FORD-10 and FORD-20 were treated exactly the same except for the differences in the exposure time of the slides (10 seconds versus 20 seconds). The subjects were fully instructed concerning the classes of material and the memory tests to be given over each class.

With one exception, to be discussed later, performance on each task was better with 20 seconds exposure than with 10 seconds exposure. Given this, we may note certain facts about the performance on each task.

1. Frequency judgments did not increase from Trial 1 to Trial 2. In FORD-10 the correlation between true and estimated frequency for the 10 states was .71 on each trial. For FORD-20, the values were .83 and .79. The difference between the two groups was also present in the raw frequency judgments as indicated by a reliable frequency by group interaction.

2. The correlations between true order of the seven streets and

judged order were .25 and .60 for the two trials for the subjects in FORD-10; the corresponding values for FORD-20 were .53 and .92.

3. Recall was 3.4% and 18.5% for the two trials for FORD-10, and 8.3% and 29.5% for FORD-20. The spacing effect was greater on the second trial than on the first, and across the two trials, performance increased more for the subjects in FORD-20 than for those in FORD-10.

We examined recall as a function of position of the item on the slide for those items occupying the same position on both slides. For the subjects in FORD-10, performance on items in the lower right portion of the slides was distinctly poorer than for items occupying other positions. This differential was not evident for the subjects in FORD-20. It seems likely that the scanning habits would result in the lower right section of the slide being the last section perceived and that it is quite possible that for some slides for some subjects the words appearing in the lower right section were never perceived with the 10-second study time. In any event, it was this finding of position differences for the 10-second exposure that led us to use the 20-second exposure on all conditions for which the results were described in earlier sections.

4. The sensitivity measure (misses plus false alarms) showed recognition performance to be better with 20 seconds of study than with 10 seconds, but the number of false alarms on the broken items did not differ for the two groups when the false alarms on new items was used as a base. That is, with the 10-second exposure, the subjects simply made more false alarms on both types of item than did the subjects having 20 seconds; there was no interaction between study time and the difference between the two types of

false alarms.

Instructions. The subjects in Conditions FORD-10 and FORD-NI had 10 seconds exposure of each slide, and differed only in terms of the initial instructions. Subjects in FORD-10 were fully apprised of the classes of items and the nature of the memory test to be given on each; the subjects in FORD-NI knew only that memory tests would be given.

The differences in instructions had surprisingly little influence on performance. In fact, frequency judgments, recall, and recognition were not reliably affected. Only performance on the ordering task was enhanced by the instructions. Whereas the mean correlations on the two trials for the subjects in FORD-10 were .25 and .60, those for the subjects in FORD-NI were -.05 and .34.

Although recall did not differ for the two groups, the spacing effect was evident for both groups, and was greater on the second trial than on the first.

Test trial. The only difference in the treatment for the subjects in FORD-NI and FORD-NIT was that in the latter condition the subjects were not tested after the first study trial. Since neither group was instructed about the tasks and tests before the first study trial, it is apparent that the first test trial provides this information. The empirical question concerns differences which may occur after the first test trial, hence performance on the second test trial for the subjects in Condition FORD-NI will be compared with performance on the only test trial for the subjects in Condition FORD-NIT. The results cannot be easily summarized since the outcome varied from task to task.

1. We have seen that in the results of many of the conditions discussed

in earlier sections, the performance on frequency judgments did not increase in precision between the two trials and some conditions showed a decrease. The results for Conditions FORD-NI and FORD-NIT indicated that in some way the process or act of being tested on the first trial was at least partly responsible. This is shown by the fact that the mean correlation between true and estimated frequency for FORD-NIT (.76) was higher than for FORD-NI (.66). In fact, the value for FORD-NIT is almost as high as that observed (.79) for FORD-20. We have not found it possible to investigate just how the act of testing was responsible for the general inability of subjects to use the information on the second study trial to improve their performance.

2. Performance on the ordering task did not differ for the two groups, the mean correlations between true order and judged order being .34 and .25 for FORD-NI and FORD-NIT, respectively.

3. Many investigators have shown that a test trial in free-recall learning improves subsequent performance. The present evidence merely supports this finding, the values being 17.0% recall for FORD-NI and 8.3% for FORD-NIT. Nevertheless, the second study trial for the subjects in FORD-NIT was not without influence, because a single study trial (for FORD-NI) produced a recall of only 3.4%

4. For the first time, the spacing effect was not given statistical support.

5. The sensitivity measure of recognition memory did not differ for the two groups (15.0% vs. 17.8%). However, the number of false alarms on the broken items did differ over and beyond the number of false alarms on the new items. The false alarms for the new and broken items were 6.7% and 15.7%, respectively, for the subjects in Condition FORD-NI. The corresponding values for the subjects in FORD-NIT were 9.7% and 30.6%. The

first test trial obviously was an important conveyor of information concerning the broken pairs. We had reported earlier that verbal instructions before the first study trial (Condition FORD-10) did not influence the number of false alarms on the broken pairs. Actually, an effect of verbal instructions was present (FORD-10 vs. FORD-NI) but fell just short of our criterion of reliability, the interaction between conditions and type of false alarm giving a p value of .02.

This completes the presentation of the experimental results. At no time have we reported on the influence of the three different retention intervals, the reason being that differences were not observed. Further, we have not reported on the Same-Different variable in recall because of the lack of any consistent effects of this variable with the 10-second exposure duration.

Correlations. We will next examine the correlational data for our three primary tasks. Reliabilities were determined by the correlations between performance on Trial 1 and Trial 2 for each task. Because Condition FORD-NIT involved only one test trial, reliabilities could not be calculated. The correlations for Condition FORD-10, for Tasks F, R, and D were .49, .64, and .48. The corresponding values for Condition FORD-NI were .27, .70, and .47. Generally speaking, these values are somewhat lower than those reported earlier (Table 9), but are sufficiently high to expect relationships between tasks to be demonstrated if such relationships exist.

The three conditions using the 10-second exposure yield nine inter-task correlations. The highest correlation observed was .29, that between recall and recognition (Task R and Task S) for Condition FORD-NIT. Even this correlation is of borderline statistical significance. We conclude that there was essentially no relationship among the performances on the different tasks when the exposure duration was 10 seconds.

Summary and Discussion

When the subject was presented four tasks to learn simultaneously, the data for all tasks were clear in showing that more learning occurred with the 20-second exposure of each slide than with the 10-second exposure. This outcome may seem so obvious as to be trivial. When the subject knew before the first study trial how he would be tested on each task, performance on three of the tasks (F, R, D) was not influenced. Only the ordering task was benefitted by the foreknowledge of the test. There is a general belief around our laboratory that when subjects are given a word list to learn, but with the nature of the learning test not specified, subjects almost universally assume recall will be requested. If this is true, foreknowledge would not influence performance on a recall test. This argument might be extended by assuming that the uninstructed subjects studied all of the classes in preparation for a recall test. There is a difficulty with this extension. If we assume that encoding for recall produces a memory that can be transformed in a direct fashion to performance on Task F and on Task D, then we would have anticipated substantial correlations among the tasks. Such correlations did not occur. Furthermore, the lack of a relationship between the performances on Task F and Task D suggests that these two tasks had little in common in the encoding.

Exposure Time and the Single Tasks

The empirical question we are asking next concerns the effect of slide exposure time on the learning of each of the three basic tasks when these are presented as single tasks as was the case for Conditions F, R, and D. The reason for asking the question stemmed primarily from the results of Condition R. In this condition the subjects were presented only the company names and the test was recall. On each trial, on the average, each name could be allotted 8 seconds of study time on each presentation, 16 seconds per trial, or 32 seconds across the two study trials. In an absolute sense, recall seemed low when viewed in terms of potential study time, recall being 30% on the first trial, 64% on the second. Furthermore, the experimenters reported that they observed subjects who did not appear to be using the full study time effectively. More bluntly, it seemed that the subjects found it difficult to attend conscientiously to the material on a slide for 20 seconds.

Having noted that there was some incidental evidence that the subjects did not effectively use the full study time for the recall task, we must point out what appears to be a contrary implication. We have seen that the addition of a second task, (either Task F or Task D) reduced recall performance by substantial amounts (see Figure 11). This finding indicates that a second task "took away" something from the recall task. One might suppose that if the subjects were not using study time fully when only Task R was presented, a second task would have little effect because, in a manner of speaking, the subject could use the time he did not use for studying Task R to study the second task. Of course, we are quite aware of the possibility that more than mere study time may be involved; it is feasible that interference may have occurred between the two tasks.

The situation seemed to call for evidence on performance on each task alone as a function of the exposure duration. In the present experiment we used exposure durations of 5, 10, and 15 seconds. We also wanted to know the relationships among our three tasks when they were learned successively rather than simultaneously. That is, we needed to know if the correlations across tasks differed when the tasks were learned successively from those we have reported for simultaneous learning. Therefore, each subject learned Task R at a given exposure duration, and then learned either Task F or Task D under the same exposure duration.

Method

Task R was always presented as the first task, and the procedure was exactly the same as that used for Condition R except that the exposure time was either 5, 10, or 15 sec. Subjects were assigned to a duration by a blocked-randomized schedule which designated 36 subjects for each of the durations. The three groups represented Condition R5, R10, and R15. As was true with Condition R, 12 subjects were assigned to each form, and were given two study and test trials.

After having learned Task R, 18 subjects (6 from each form) were given two trials on Task F and 18 were given two trials on Task D. The exposure duration for the second task was the same as for Task R for each group. For all tasks, the subjects were fully informed concerning the materials and the memory tests to be given.

Results

Recall. The number of items recalled increased directly as exposure duration increased. For the first test trial the values were 16.5, 21.8, and 25.9%, for R5, R10, and R15, respectively. On the second trial the values were 42.4, 53.2, and 55.3%. Performance with a 20-second exposure (as reported for Condition R earlier) was well within the range to be expected if the above values were extrapolated to predict performance at 20 seconds. The recall values, with 20 seconds exposure were 30.0% and 64.4% for trials 1 and 2.

The effect of spacing the two occurrences of the items is shown in Figure 15. The values represent the mean percent correct for the two trials. Again, the spacing effect was highly reliable ($p = .001$), and although Figure 15 suggests some variations as a function of exposure duration, the interaction was far from being reliable statistically. Indeed, no interaction among the variables (trials, spacing, exposure duration, or same-different) was reliable. The Same-Different variable had a small effect ($p = .02$), the recall being 37.3% when the item occupied the same spatial position on its two occurrences, and 34.4% when the position differed.

The manipulation of exposure duration as carried out in this experiment confounds duration and length of retention interval. The longer the exposure duration the longer the retention interval for all items except the last one in the study list. All of the data presented earlier have shown that differences in performance have not occurred as the retention interval increases up to several minutes. Nevertheless, none of these determinations involved tasks learned singly. It seemed necessary, therefore, to ask about recall as a function of position in the study list. We formed five groups of six items each based on the positions of second occurrence in the study list (just as was done for the data from Condition FORD and reported earlier), and asked about recall as a function of the five groupings. Because the results were much the same for each trial, we have combined the data for the two trials for presentation in Figure 16.

The differences in the length of the retention interval as a function of exposure duration would be maximal for items holding the early positions in the list. As may be observed in Figure 16, recall of the items in the initial position is far better with the longer exposure durations (10 and 15 seconds) than with the 5-second exposure. Of course, there is no way to assert from these data that forgetting was not greater for the items in the initial study positions for the longer study times than for the short. But we can conclude that if the forgetting did differ, it was not of sufficient magnitude to negate the higher level of learning achieved with the longer exposure time than with the short. On the other hand, it might be argued that the greater recency effect associated with the longer exposure periods than with the short indicates that the retention interval was of some consequence for the initial items, assuming that the learning which occurred was equivalent for both primary and recency items. The retention-interval differences would be less for the recency items than for the primacy items. We see no way to arrive at definitive conclusions on this matter, but because of the lack of effects of the length of the retention interval in previous data, intervals which differed as greatly as those here, we are inclined toward the position that the data represent a reasonable estimate of the effects of exposure duration on recall. We do not believe there is a serious diminution of the differences produced by differences in the lengths of retention intervals.

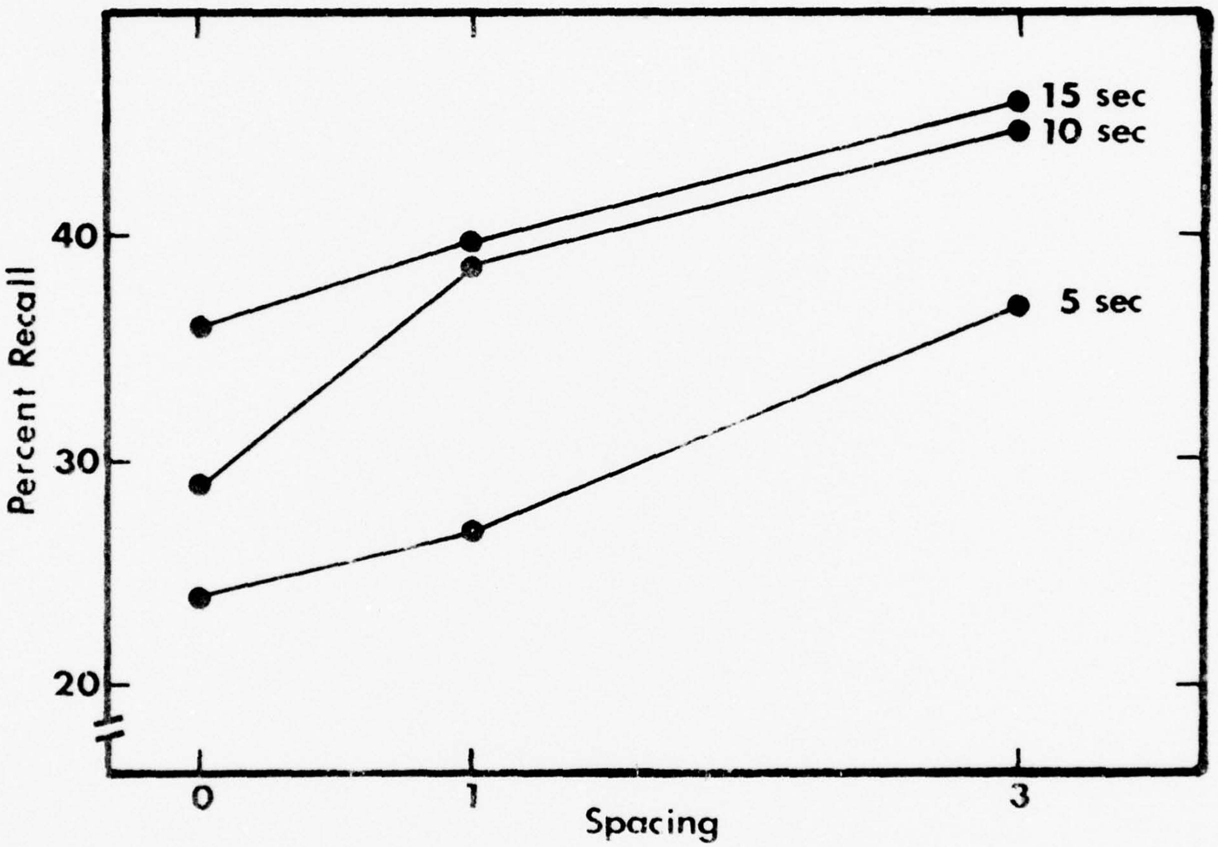

Figure 15. Recall as a function of exposure duration (5, 10, 15 sec.) and spacing.

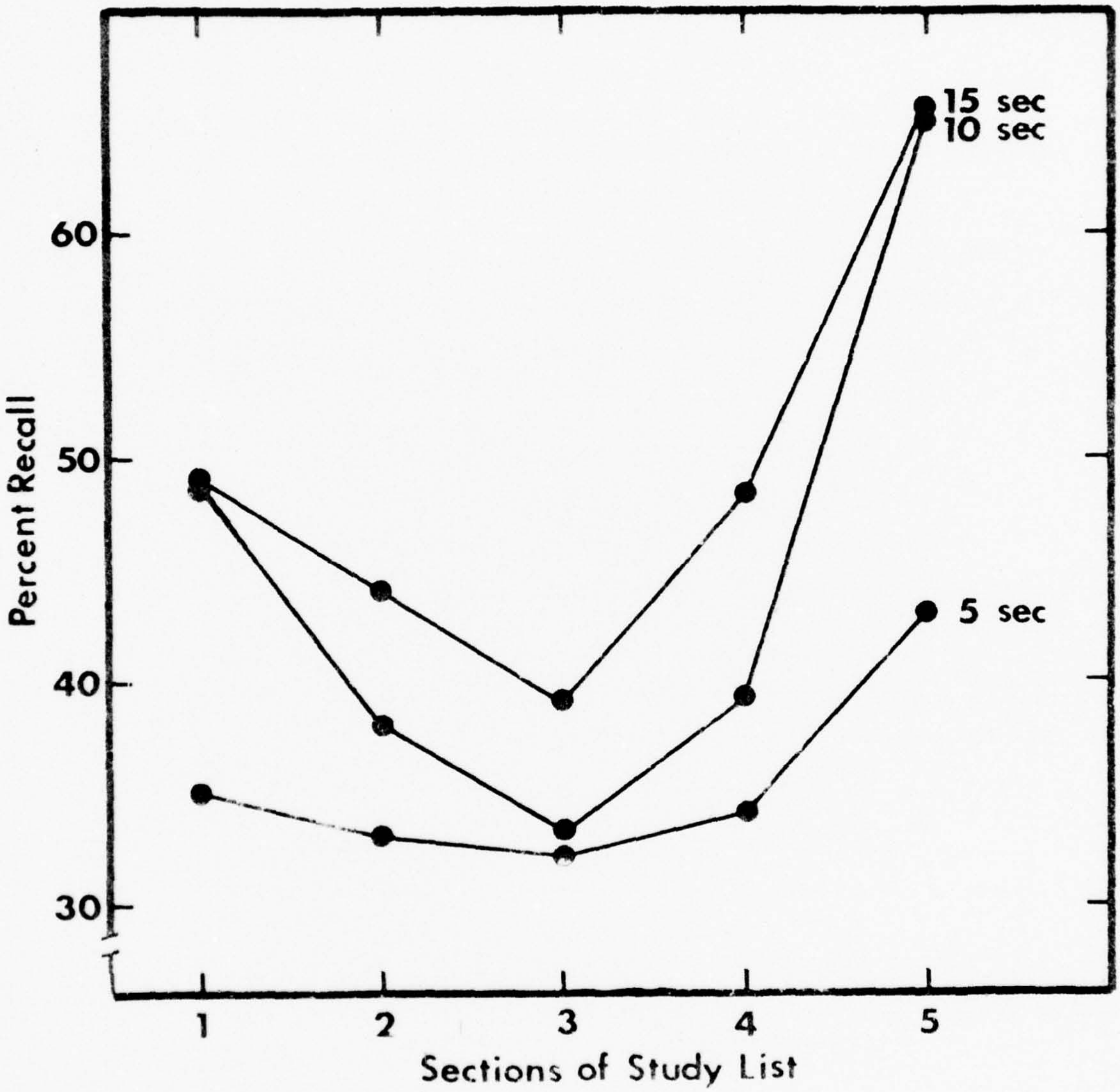

Figure 16. Recall as a function of position of items in the list (Sections 1 through 5) and exposure duration (5, 10, 15 seconds).

Frequency judgments. After the 36 subjects in each group had completed the second recall trial, 18 were given Task F and 18 were given Task D, using the exposure duration that was used for recall. Thus, the conditions were identical to Conditions F and D as reported earlier, except the exposure durations were less.

Performance on frequency estimation was extremely good under all conditions. The mean correlations (as determined from retransformed $\underline{z}'$ values) on the first trial were .88, .93, and .92 for 5, 10, and 15 seconds, respectively. For the second trial, the corresponding values were .93, .96, and .96. Although the performance with 5 seconds exposure was a little lower than that with the longer durations, a statistical analysis showed only the trial effect to be reliable. The values recorded earlier for performance with 20 seconds of study time per slide were .94 and .95 for the two trials in order. A plot of the mean frequency judgments as a function of true frequency on the second trial produced curves that were almost identical to that shown for Condition F in Figure 10. The evidence is conclusive that the estimation of the frequency with which the state names occurred was not enhanced by study time beyond 5 seconds on the first study trial despite the fact that further study on the second trial improved performance. This might suggest that in order to improve performance with additional study time a subject must be tested, but we have given evidence earlier that testing may interfere with further learning of the distribution of the frequency of events. However this situation is viewed, it appears that for our particular 10 events the subjects essentially "wasted" 15 seconds when the exposure was 20 seconds.

Recognition. Again, the measure of sensitivity used was the sum of the misses on old items and the false alarms on new items. The percentage values are shown in Figure 17, and we have included the results for the 20-second exposure period, given earlier as Condition D. As may be seen, the results correspond closely to those given for frequency assimilation. The length of the exposure period beyond 5 seconds was irrelevant to performance on the first trial in spite of the fact that a second study trial improved performance sharply (p = .001).

The number of false alarms on the broken items was again unrelated to the separation of the two words on the study trial. The effect of exposure duration on the number of these false alarms was somewhat ambiguous. Summed across the two trials the values were 8.1%, 8.3%, and 2.3%, for 5, 10, and 15 seconds, respectively. However, the value for Condition D (described much earlier), which had an exposure duration of 20 seconds, was 10.4%. There is, apparently, no systematic trend in the number of false alarms as the exposure duration increased. We are inclined, therefore, to discount the significance of the small number observed with the 15-second exposure duration. In all conditions, the number observed was greater than the number of false alarms on the new items (p = .001), and the interaction between conditions and type of false alarms did not reach an acceptable level of statistical reliability.

Correlations. The intertask correlations did not show any systematic relationship with exposure duration. The three correlations between the performances on Tasks R and D (each based on 18 subjects) were .42, .21, and .57, for Conditions R5, R10, and R15 in order. The average of these three

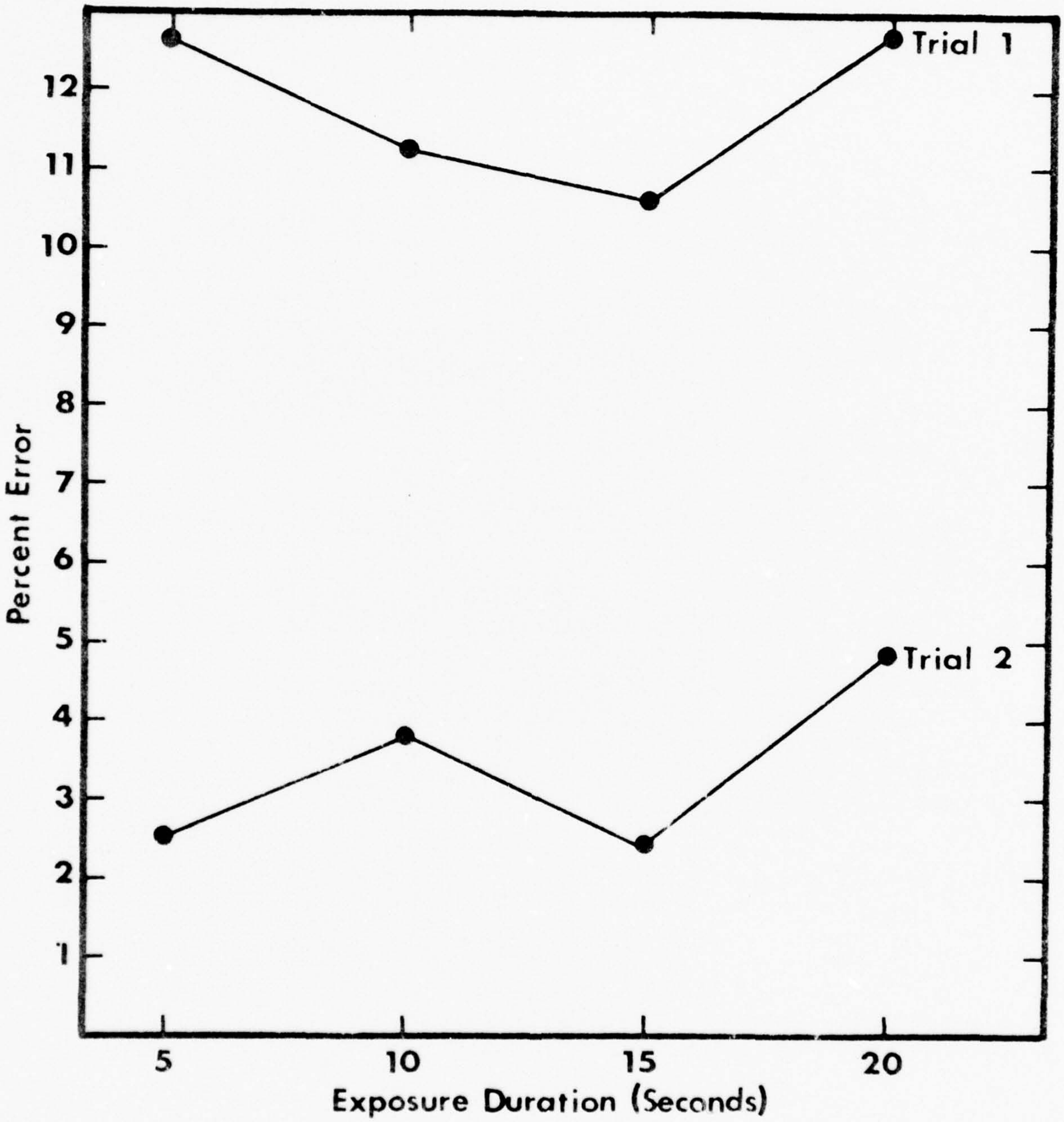

Figure 17. Percent recognition errors (misses plus false alarms) as a function of exposure duration and trial.

is almost identical to the average of the conditions in the main experiment as seen in Table 10. The three correlations between the performances on Task R and Task F were .37, -.05, and .30. Although these values are, on the average, a little higher than those shown in Table 10, in neither case can they be said to differ reliably from zero. Essentially, then, the inter-task correlations which were observed when the subjects learned the tasks sequentially did not differ from those which were observed when the tasks were presented for simultaneous learning. We cannot, therefore, hold to the notion that independent encoding occurs only when a subject is faced with the learning of two or more tasks simultaneously. The independent encoding is just as likely to occur when the tasks are learned at different times.

Summary and Discussion

The results of this experiment have shown that recall is directly related to exposure time between 5 seconds and 20 seconds, but that neither frequency estimation nor recognition decisions are influenced by exposure time. Our major purpose here will be to try to put these findings in perspective.

The first point we would like to emphasize is that although recall increases reliably as study time increases, the amount of this increase is small in an absolute sense, and that any notion of efficiency in learning would recommend the use of the shorter study period. We will look at this from three points of view. On the second recall trial the mean numbers of items recalled were 12.7, 16.0, 16.6, and 19.3 for 5, 10, 15, and 20 seconds, respectively. The corresponding total study times were 240, 480, 720, and 960 seconds. On the average, each correct response consumed 19, 30, 43, and 50 seconds of study time. That is, with 5 seconds exposure time, a correct response was based on 19 seconds of exposure time, with 20 seconds exposure, the value was 50 seconds. Even allowing for the fact that with greater recall, more difficult names are involved, the exposure time required to produce a correct response is much higher for the long exposure times than for the short.

As a second way of viewing the matter, we ask about the *performance* which would have been observed had there been further study trials. Performance on trials 1 and 2 were used to project a straight line to determine on what later trial all 30 items would have been recalled. Of course, this is not appropriate because free-recall learning curves are markedly negatively accelerated, but the influence of the inappropriateness should be roughly equivalent for the different conditions. For the 20-second exposure time, the projection showed that three trials would be required, or a total of 24 minutes of study time. With the 5-second exposure period, the extrapolation indicated that slightly over four trials would be required to yield perfect performance, or a little over 8 minutes of study time.

Finally, we may look at this in a third way. With the 20-second exposure period, 960 seconds of study time were given. With a 5-second exposure period, 8 study trials could be given within 960 seconds. Although this doesn't include differences in test times, it seems beyond doubt that if only a single final test trial was given in both cases, recall would be higher for a group having 8 trials at a 5-second rate than would a group having 2 trials at a 20-second rate.

We are now prepared to consider a puzzle. To do this, we must first look back at recall under three conditions for which the results were reported earlier (Table 5). For the two trials combined, the recall values for Condition R, FR and RD were 47%, 29%, and 26%. Using rough figures, it can be seen that when the Task R and either Task F or Task D were learned simultaneously, the recall performance dropped by 20%. When only Task R was involved, recall fell about the same amount when the exposure time was reduced from 20 seconds to 5 seconds -- from 47% to 29%. Yet we have seen that exposure periods beyond 5 second had no influence on either recognition or frequency estimation (Task D or Task F). In a simple-minded way, it would seem that when Task R was presented for simultaneous learning with either Task D or Task F, the subject could allot 5 seconds of each 20-second exposure period to Task F (or Task D) and use the remaining 15 seconds to study the company names in preparation for recall. If all this works out in this way, performance on Task F or Task D should be comparable to the performance observed when these tasks were presented alone. Recall should be equivalent to that observed with the 15-second exposure duration when Task R was presented alone. This value would be 41%.

Obviously, the simple-minded approach is not the correct one. Something changes when two or more tasks are presented for simultaneous learning, and at this point we cannot tell just what is involved. It would not appear that a major factor would be the time lost in merely identifying stimuli (which increase as the number of tasks increase), because this amount of added time within the ranges with which we are dealing does not seem to be a highly important factor.

DIVISION OF EFFORT

We have assumed that in some sense there must be a division of effort when a subject attempts to learn several tasks simultaneously, as in Condition FORD. It is obvious that when three or four tasks are given simultaneously, the subjects must shift from one class of materials to another. These shifts could impede learning because the subjects take time to classify each stimulus even though to do so is of no consequence for learning. Or, because the subjects control their study time within a slide, it may be that they spend too much time on items they have already learned, and not enough time on those which they have not learned. Or, as a third possibility, the subjects may miss some of the stimuli entirely.

The purpose of the final experiment was to minimize the influence of the subjects' control of within-slide study time. In so doing, we asked a most general question about simultaneous learning, namely, does the simultaneous-learning paradigm impede learning, facilitate learning, or have no influence. In the critical condition, the subjects saw all of the stimuli for one of the tasks on the study trial, then all for a second task, and finally, all for the third task. Then, the appropriate memory tests were given. The total study time was the same as for Condition FRD, but the subject never had to study more than one task at a time. We will designate this as Condition F-R-D to indicate that the three tasks were studied sequentially as opposed to simultaneously in Condition FRD.

A third condition was added. Under Condition F-R-D, not only were the subjects shown all stimuli of a given class before moving on to the next class, but they also saw only a single class of stimuli on a slide. We needed to remove this latter confound, and to do so we arrayed the slides so that each slide carried stimuli of only one class, but the class varied from slide to slide in a random manner. We have called this arrangement Condition FRD(S), where S refers to the appearance of a single class of stimuli on a slide. Thus, the three conditions were FRD, FRD(S), and F-R-D.

Method

Slides. It will be remembered that for Conditions F, R, and D, as presented in earlier sections, the subject was presented slides on which only the state names appeared (Task F), only company names appeared (Task R), or only traffic signs appeared (Task D). In implementing Condition F-R-D for the current study, the three sets of slides used for Conditions F, R, and D were simply given in sequence (including the primacy and recency slides for each task). Therefore, the subjects were shown 78 successive slides. As will be explained later, the order in which the three tasks was given was varied across subgroups of subjects.

For Condition FRD(S), it was necessary to present the same 78 slides as shown in Condition F-R-D. The three sets of slides (26 for each task) were interwoven in such a way that the first three slides (after the primacy slides) carried the same stimuli as did the first slide for Condition FRD. In the same way, slides 4-6 carried the same stimuli as did the second slide for Condition FRD, and so on. Within each group of three slides for Condition FRD(S) the order was random, subject to the restriction that two consecutive slides not carry stimuli from the same class. Thus, the last slide of one set of three had to carry a different class of material than did the first slide of the following set of three. Of course, different slide orders were constructed for each of the three forms for Conditions F-R-D and FRD(S).

Procedure and subjects. Each of the 78 slides required for Conditions F-R-D and FRD(S) was presented for 7 seconds, while each of the 26 slides used for Condition FRD was shown for 20 seconds. Obviously, the total study time per trial was 26 seconds longer for Conditions FRD(S) and F-R-D than for Condition FRD (546 seconds versus 520 seconds). We have assumed that this small difference will be of little consequence.

The order of the three tasks for Condition F-R-D on the study trials was varied three ways such that each task occurred equally often in each of the three positions. The order of the retention tests was also varied three ways and these were orthogonal to the three orders of study, and were also crossed with forms. A total of 54 subjects was assigned to each of the three conditions.

The subjects were fully informed concerning the nature of the study lists and of the nature of the tests to be given for each class of material. Furthermore, each subject was given a card which described briefly the relationship between class of stimuli and type of retention test; e.g., company names were to be recalled. The subjects had this card available at all times throughout the session, and they were told to refer to the card in case they forgot how they were to be tested for a given class of material.

Results

Task F (frequency judgments). The correlation between the judged frequency of the 10 state names and their true frequency was determined for each subject. The z' transformation was again used for statistical purposes. The mean correlations are shown in Figure 18 as a function of trial and conditions. The three conditions constituted a reliable source of variance (p = .001), and, as can be seen, this was largely produced by the better performance of the subjects given Condition F-R-D than by those assigned to the other two conditions. In spite of the statistical significance of the differences among conditions, we should point out that in any absolute sense the differences are small, and that the correlations were high for all conditions. When the raw frequency judgments were used as the response measure, the differences among the conditions were not statistically reliable. The critical statistic was the interaction between condition and presented frequency. The sharpness of the slope for each condition should correspond to the height of the line in Figure 18. The slope differences did correspond to the differences in Figure 18 but were not reliable statistically (p = .06). Thus, the two response measures did not agree in statistical detail. It seems to us that the appropriate conclusion is that a small effect of conditions was present, and that performance under Condition F-R-D was better than that under the other two conditions.

It will be remembered that in Condition F-R-D each task occurred in each of the three positions in the study list an equal number of times, and that 18 subjects were assigned to each of the three study orders (FDR, RFD, DRF). Because each retention interval occurred with each order an equal number of times, differences due to position of the task in the study list could be assessed directly. Neither response measure reflected an influence of the position of the task in the study list, indicating again the stability of the memories across short retention intervals. Differences in the length of the retention interval produced by differences in the order of taking the three tests again also failed to influence performance.

Task R (recall). An analysis of recall showed that conditions did not differ, although the influence of spacing was apparent as usual (p = .001). Figure 19 shows the percent recall as a function of condition and lag for both trials combined. There was inconsistency in the magnitude of the lag effect as a function of conditions (p = .004). This interaction was present on both trials, and we have been unable to find any correlates of it, or elucidate upon it in any way. The Same-Different variable did not produce a reliable main effect and it did not enter into any reliable interactions. The critical finding for the present purposes is the lack of a difference in recall among the three conditions. Overall, the recall values were 24.2, 21.7, and 23.9% for Conditions FRD, FRD(S), and F-R-D, respectfully.

The length of the retention interval did not influence recall. Summing across the three conditions for both trials gave values of 23.1, 23.6, and 23.1% for the short, medium, and long retention intervals, respectively. Condition F-R-D was analysed both for the position of the task in the study list and for the length of the retention interval. Neither of these variables influenced recall. The recall values when Task R was in the first, second, and third positions in the study list were 22.6, 23.3, and 25.8%. The F was less than 1. The longest retention interval would be the case in which the task occupied the first position in the study list, and had the long retention interval. The recall for this combination was 26.7%. The shortest retention interval would be the case where the task occurred in the final position in the study list and was recalled first. The value for this cell was 22.5%. We emphasize this lack of differences to press the point that even when the retention interval was extended appreciably in length over any previous intervals used, recall still remained relatively uninfluenced as the retention interval grew longer and longer.

<u>Task D (recognition)</u>. Differences in recognition performance across

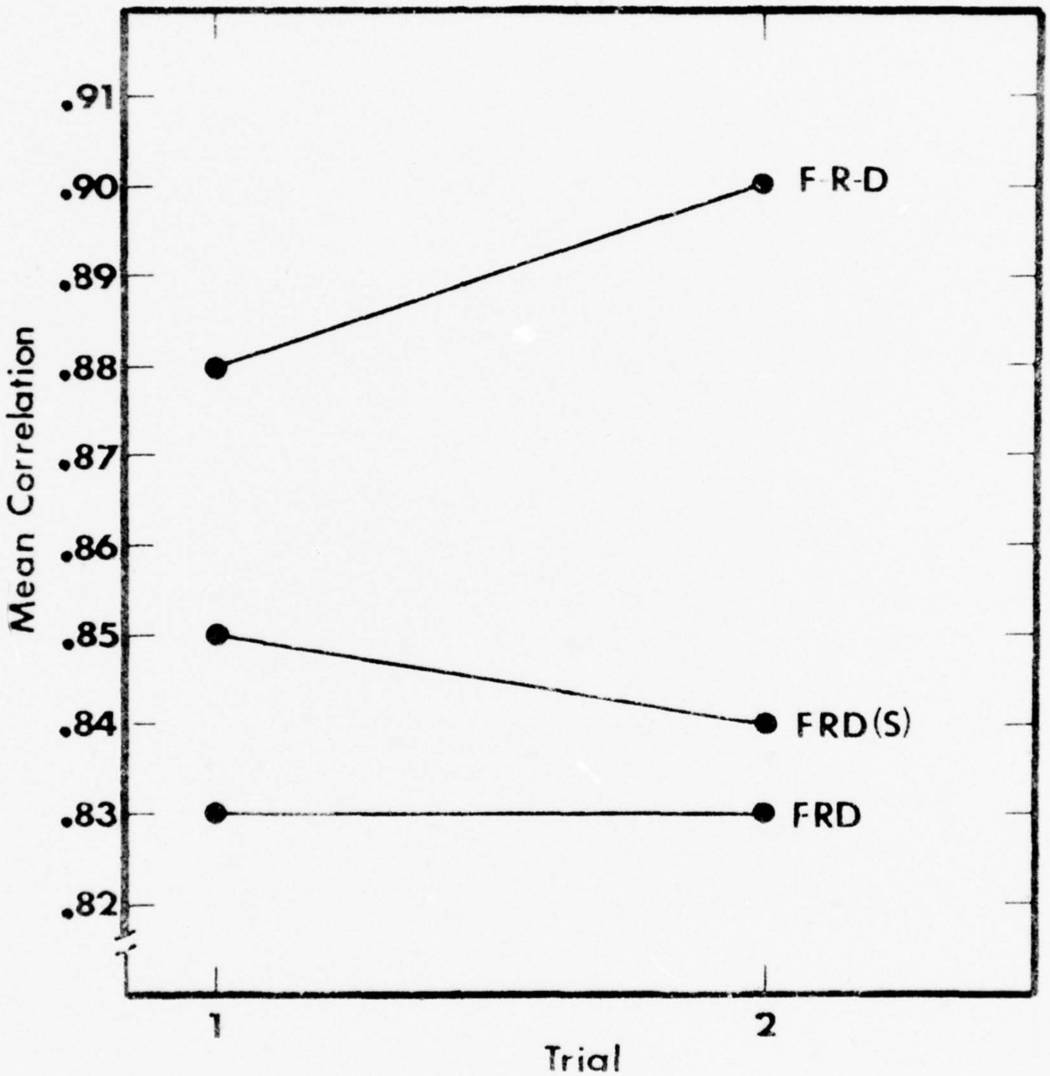

Figure 18. Mean correlation between true and estimated frequency for each trial for each condition.

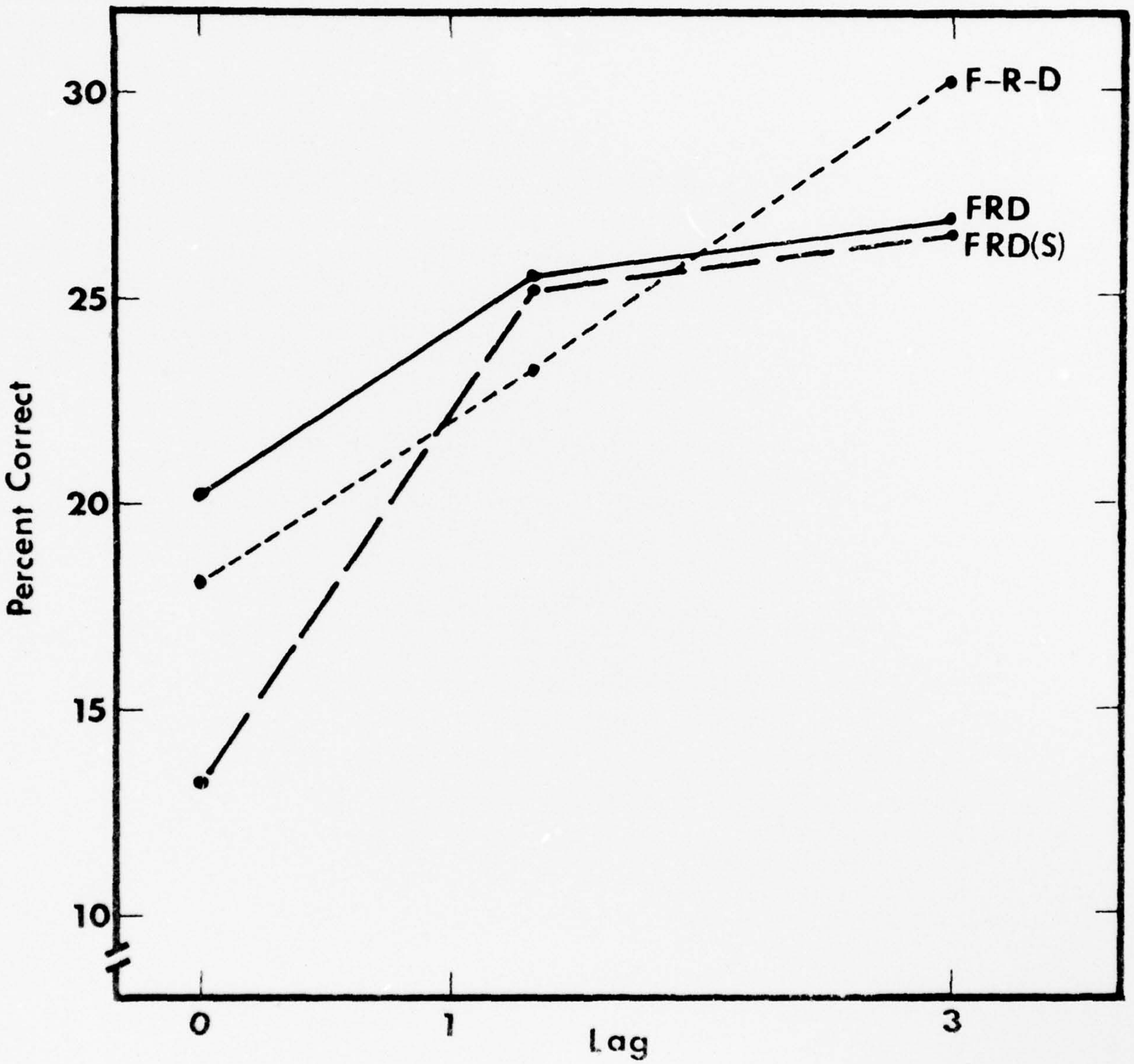

Figure 19. Recall (both trials combined) as a function of lag and conditions.

the three conditions occurred on only one type of item; consequently we will dismiss quickly those types for which differences did not occur. As has been true throughout, errors on broken pairs exceeded those on new pairs, but the combined errors on the broken and new pairs did not differ across condition (p = .34), and the length of the separation variable had no influence (p = .70).

The only reliable effect of conditions is shown in Figure 20, where misses and false alarms (on new items) summed across both trials are shown for each condition. The interaction between condition and item type was highly reliable (p = .001), as was also the difference among conditions on total errors (p = .001). No other factor was significant statistically nor interacted with conditions. For Condition F-R-D no effect was found for the position of the recognition task in the study list.

Correlations. Trial 1 by Trial 2 correlations showed substantial reliability for all tasks for all conditions. As has been true for previous data, the values used for the correlations were total errors for Task D, z' transformation of r for Task F, and total recall for Task R. The nine correlations between trials 1 and 2 varied between .44 and .84. The means of the three correlations were .61, .74, and .51 for Conditions FRD, FRD(S), and F-R-D, respectively. By conventional statistical tests, the two extreme means do not differ statistically. In earlier sections we considered the likelihood of subjects attending differentially to the three tasks on the two trials. In particular, it seemed that some subjects might spend more time in studying the company names on the second trial than on the first, thereby lowering the intertrial correlations for all tasks. The present data give no support to this idea. Under Condition F-R-D it would be difficult to rehearse across tasks, yet the reliabilities for this condition were lower than were those for Condition FRD where the subject could allocate time to tasks differentially on the two trials. Our earlier concern about differential attention does not now seem to have been justified. The average intertrial correlations for Tasks F, R, and D were .65, .65, and .54, respectively.

We turn next to the correlations of tasks within each of the three conditions. Supporting our previous findings, we found that the performances on Task R and Task F were essentially unrelated, the three correlations being .16, .14, and .07. The three correlations between Task R and Task D were .11, .56, and .36 for Conditions FRD, FRD(S), and F-R-D, respectfully. We do not know why the correlation was so low for Condition FRD. This condition was used earlier and the correlation was .27 (Table 10), and for a very similar condition (FRD-O), the value was .40. The correlations between Task F and Task D were .30, .37, and .07 for Conditions FRD, FRD(S) and F-R-D in order. The first two correlations are quite in line with those found earlier (Table 10), and although the third correlation might seem to be out of line, it cannot be judged to be so statistically. All in all, we have not found the correlations for this experiment to add much to our knowledge.

Summary and Discussion

We conclude that differences in performance observed for Condition FRD (simultaneous learning) and Condition F-R-D (sequential learning) were minor. The two conditions did not differ in recall. Frequency assimilation

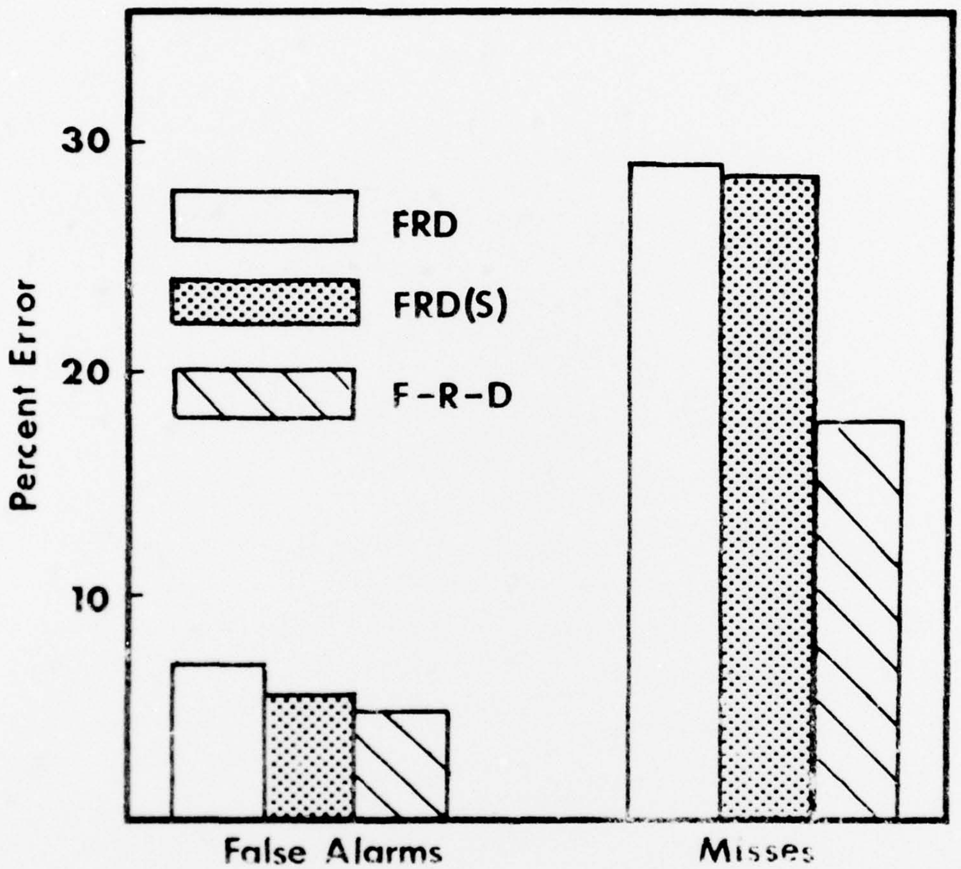

Figure 20. Misses and false alarms in recognition for the two test trials combined as a function of conditions.

was probably a little better for sequential learning than for simultaneous learning. For recognition, the groups differed on one type of error (misses). We do not believe that any point of fundamental significance can be made from these differences, which is to say that it appears that simultaneous learning of three tasks occurred at about the same rate as did the learning of each task separately. That the subjects may have to "jump" from one class of materials to another seems to have only a small negative effect at the worst. The fact that performance on all tasks was essentially equivalent for Conditions FRD and FRD(S) indicates that having different classes of material within a slide is of no consequence for the learning.

GENERAL SUMMARY AND DISCUSSION

Our empirical knowledge of human memory has evolved primarily from experiments in which single, simple tasks have been used as the experimental vehicles. In the central conditions of the present report we required that our subjects learn four different tasks simultaneously. The evidence which has resulted does not demand that we qualify at length our knowledge gained from the use of single tasks. Generally speaking, the effect of variables built into our individual tasks was much the same as has been found when the single task only has been learned. To be presented with verbal materials for four different tasks (defined in terms of the verabal materials used and the retention tests taken) may have confused the subjects, but if it did, it did not prevent the occurrence of basic memory phenomena, e.g., the spacing effect. In fact, presenting three tasks simultaneously gave much the same learning as that which occurred when the tasks were presented separately.

The incidental learning which we observed did not allow the conclusion which can sometimes be drawn following the use of more conventional techniques for studying incidental learning. For example, whereas other investigators (e.g., Flexer & Bower, 1975) have concluded that frequency assimilation is as high under incidental conditions as under intentional conditions, this was not the case in our data. The incidental learning for all four tasks was always lower than that for intentional learning, and in an absolute sense was not great in amount for any of them. Still, it must be recognized that the small amount of incidental learning which did occur within a few minutes of intentional learning would, when we think in terms of hours and days of incidental exposure to events, constitute a formidable proportion of our relatively permanent memories.

Perhaps the most unexpected finding had to do with the stability of the learning when the several tasks were learned simultaneously. There was no forgetting over several minutes. This was true in spite of the fact that the retention intervals were filled with tests on other tasks. We do not in fact know that this lack of forgetting occurred only under conditions of simultaneous learning. We did not test the retention of these tasks when learned individually. Nor do we know that our findings have generality across other types of material which might be used in simultaneous learning. Nevertheless, the possibility that simultaneous learning of several tasks may retard the forgetting of each task is one that seems to warrant further work. A study by Burnstein (1962) would also seem to recommend this further work. Burnstein has his subjects learn 10 three-place numbers (Set L) while not learning 10 other three-place numbers (Set NL) that occurred in the series. After 60 minutes, retention of the numbers in Set L was superior to the retention of this set by members of a control group that had learned Set L without the items of Set NL being present in the list.

The low correlations across tasks could be interpreted to mean that the acquisition processes for the different tasks had little overlap. At one point in our work these findings suggested the possibility that the subjects deliberately encoded the tasks differently to meet the demands of the tests. The subsequent research negated this possibility because the correlations were also found to be low when the tasks were learned separately. This finding could mean that the subjects encoded the tasks differently, but if this is true, it was not because several tasks were being learned simultaneously. A further caution should be added at this time. Type of retention test and type of material were confounded throughout our work. For example, we did not use traffic signs in recall tests, nor did we use the company names in studying frequency assimilation. Furthermore, the number of verbal units in each task differed. The lack of correlations among the scores on the tasks may be due to differences in the materials, not to the differences in the type of retention tests. Our work was simply not analytical with regard to this.

Some of the most perplexing data related to study time. Five seconds of study time per slide on single tasks produced as much learning for recognition and for frequency judgments as did 15 or 20 seconds. Yet, a second trial markedly reduced the errors made on the recognition test. Why did the subjects not "use" the time on the first trial beyond 5 seconds to study the items if they subsequently studied it further when it was presented on the second trial? Furthermore, consider again the fact that the subject apparently gave no more than 5 seconds of study to a slide when more was available. Why, then, would a second task (frequency judgments) which also does not improve beyond 5 seconds of study when presented alone, cause a decrement in recognition performance when the two tasks are learned simultaneously with each slide being presented for 20 seconds? It would appear as if 10 seconds could go unused and still performance would be much the same as that observed for the single task. This did not occur.

Until these puzzles are given further experimental examination, two matters may admonish caution. First, we have interpreted the fact that intertrial reliabilities remained fairly constant (regardless of the number of tasks being learned) to mean that subjects did not trade off study time among tasks. Our argument was that if such trade-off practices occurred, the intertrial correlations should be lowered as the number of tasks increased. We were therefore led to the supposition that the subjects studied all items on a slide in a fairly consistent way from trial to trial. The second matter results from questioning a basic assumption we have made about study time. We have concluded that because the subjects did not improve in performance on the single task as study time increased beyond 5 seconds, they therefore did not study at all during the exposure period beyond 5 seconds. This may not be true. The subjects may have behaved as we believed they did under simultaneous learning of several tasks. That is, they used the full study period in trying to learn, but what they learned beyond 5 seconds was of no consequence for performance on recognition tests or on frequency judging tests. Just why they improved on the second trial for the recognition test would require some additional assumptions which we have not developed.

We have tended to reach conclusions which indicate that the simultaneous acquisition of several tasks does not produce any fundamentally different explanatory problems than does the learning of a single task. However, some of the puzzles arising in conjunction with the results for study-time differences may advise us to maintain some reservations. For example, consider the case mentioned earlier. Recognition performance and frequency judging performance did not improve with exposure periods beyond 5 seconds. Yet, when both tasks were learned simultaneously with a 20-second exposure, performance was poorer than when each was learned singly. It is almost as if a synergistic reaction occurs when two or more tasks are learned simultaneously, and the laws of study time which hold for the single task may not always fit the laws which will emerge from the simultaneous learning of two or more tasks.

One other finding is related both to study-time effects and to differential encoding. When two or more tasks were learned simultaneously, the presence of a task to be recalled degraded performance on the other tasks more than if the recall task was not involved. Furthermore, recall performance increased directly as study time increased from 5 through 20 seconds when the recall task was presented singly. Do these two facts mean that encoding for recall is different from encoding for frequency judgments and for recognition? It could mean that, but it need not. We could assume that encoding is precisely the same for all tasks whether learned singly or multiply. The study or encoding which takes place beyond 5 seconds of study time benefits recall but does not benefit recognition or frequency judgments. But if this is true, why should it be true? What is the nature of the information which is acquired in the period beyond 5 seconds which facilitates recall but has no benefit for recognition? To even speak in these terms comes close to suggesting that there were encoding differences after all.

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